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VISUAL GUIDANCE IN MOTOR LEARNING

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A survey of the vast literature upon the learning process reveals a wide variance of opinion concerning its ultimate nature.[1] Many are the theories that have been propounded to explain it. At one extreme may be placed those that make learning entirely a matter of trial and error—no difference in kind between human and animal learning being admitted;[2] and at the other extreme may be placed those that admit a difference and account for certain forms of learning as dependent upon what has been called 'insight.'[3]

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[1]Because of the pressure for space the historical survey is omitted. The reader is referred to E. L. Thorndike's (*Educational Psychology*, 1913, vol. ii, Chaps. 7 and 8), and W. S. Hunter's (*Foundations of Experimental Psychology*, 1929, Chap. 15) reviews, and to the annual summaries of the literature in the *Psychological Bulletin*.

[2]Cf., for an example of this extreme view, J. F. Dashiell's account (*Fundamentals of Objective Psychology*, 1928, 333). He writes that "man's learning is like that of a cat or monkey in that it is a matter of stimuli and responses; where it differs from the latter is largely in the degree of refinement of the stimuli and in the degree of implicitness of the responses."

[3]This concept was introduced into psychology by W. Köhler (*The Mentality of Apes*, 1925), and it has since been widely accepted. K. Koffka (*The Growth of the Mind*, 1924, 214) explains it as follows: "The sudden grasping of the solution which results, is a process that runs its course in accordance with the nature of the situation, so that the complete solution of the problem takes place with reference to the configuration of the field of perception." R. M. Yerkes has also adopted the term. He says that he uses 'insight' "to designate varieties of experience which in us are accompaniments of sudden, effective, individually wrought adaptations to more or less distinctly new and problematic situations." (The mind of a gorilla, *Genet. Psychol Monog.*, 2, 1927, 155).

Problem

The object of the present study is to determine whether different types of learning existed. We wished to set up conditions that would permit learning with and without 'insight' and to compare the performances in the two cases. To this end we used motor learning and the pencil-maze. We used motor learning because variations in the experimental procedure may be readily made; and the maze because it has repeatedly demonstrated its usefulness in studies of this type.

A great deal of work has been done concerning the nature of the human response in the maze-situation.[4] In some experiments knowledge of the situation has been limited to the kinaesthetic experiences, in others this has been supplemented and reënforced by additional cues. The cues most generally used have guided the subjects, have lessened their dependence upon trial and error, and have permitted them to utilize a rationalized mode of attack. The cues may, therefore, be regarded as a means of facilitating 'insight.'

The majority of investigators employing supplementary cues have used some form of visual guidance. Perkins,[5] in an elaborate experimental set-up gave his subjects visual cues by having them look through a tracing tube, 1 in. in diam. and 10 in. long. "All of the field except the small range allowed by the circular opening, was cut off and the subject could see none of the field except the part of the path which lay directly at the bottom of the tube." Smith[6] likewise constructed his apparatus so as to permit his subjects a limited view of his maze. Carr used two methods:[7] in the first the maze was constructed so that the blinds, though the entire pattern was visible, were hidden from view; in the second he placed a map of the maze before his subjects. Miles devised a two-story duplicate maze, in which the subject not only saw the problem, but traced through the duplicate with one hand while performing the learning problem with the other;[8] thus receiving supplementary kinaesthetic as well as visual guidance.

Guidance adds considerably to the subject's knowledge about the problem, and may well, therefore, be said to permit 'insight' into its solution.

Specifically, the present study attempts to determine the effect of one type of visual guidance in maze learning. This type of guidance involved 'undirected intention;' that is, the subjects visually perceived others run the maze without knowing that they would, at a subsequent period, be called upon to solve it themselves.

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[4]H. A. Carr and V. C. Hicks, Human reactions in a maze, *J. Animal Behav.*, 11, 1912, 245-253.

[5]N. L. Perkins, Human reactions in a maze of fixed orientation, *Comp. Psychol. Monog.*, 4, 1927, (no. 21), 1-92.

[6]F. O. Smith, Differential reactions of human beings in the maze, *Ped. Sem.*, 34, 1927, 394-405.

[7]H. A. Carr, The influence of visual guidance in maze learning, *J. Exper. Psychol.*, 4, 1921, 399-417.

[8]W. R. Miles, The two-story duplicate maze, *J. Exper. Psychol.*, 10, 1927, 365-377.

Method and Procedure

Apparatus. A modification of Young's Slot Maze A was used,[9] the pattern of which is shown in Fig. 1. Young's pattern was not changed; the modification simply made possible its use as a pencil maze.

The maze consisted of a block of wood 25 x 26 cm., over which was fitted a smooth jacket into which the maze pattern was cut. The metal jacket was about 3 mm. thick, and it was cast with flanges which fitted snugly over the edges of

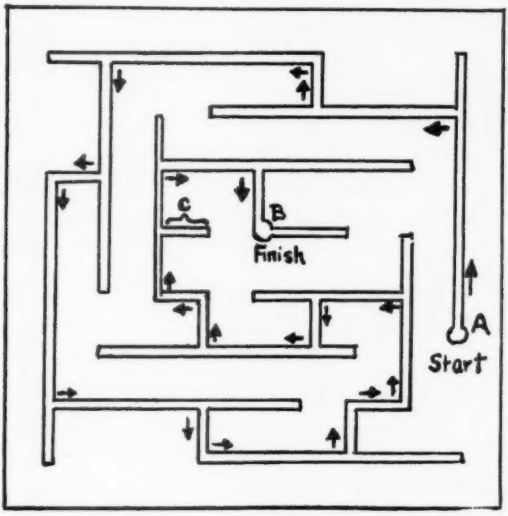

Fig. 1. Pattern of the Young Slot Maze A

the wooden base thus assuring constancy in the relative position of the two parts. By simply lifting the jacket, a piece of paper, cut to proper size, could be placed between them. The paper formed the floor of the maze and the surface upon which the *S*'s run was recorded. A hole was bored into the wooden base at the goal, consequently *S* could tell when he reached it by the fact that the pencil broke through the paper and plunged into the hole. The pathways of the maze were about 5 mm. in width, consequently only one side at a time could be followed by the pencil. This point should be noted because it, in contrast with the type in which the stylus is the width of the groove, adds to the complexity of the problem. The maze used in this experiment included 6 of the 10 desirable features listed by Knotts and Miles.[10] The features omitted were ones that could not be used in the present study.

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[9] H. H. Young, Slot maze A, *Psychol. Clin.*, 14, 1922, 73-82.

[10] J. R. Knotts and W. R. Miles, Notes of the history and construction of the stylus maze, *Ped. Sem.*, 35, 1928, 415-425.

As can be noted in Fig. 1, the pattern is such that the pathway constitutes a continuous groove from A to B with all changes in direction being right angle turns. The culs-de-sac, with one exception, c, are continuous with the direct pathways. The distances between turns and the directions of the turns are variable, so that there is no specifically related sequence in the order in which these phases of the pathway present themselves. The pattern allows for only one pathway between the start and the finish.

A pencil was used as a stylus and suitable material to make a secure and light-proof blindfold was provided.

Subjects. Three hundred undergraduate students (67 men and 233 women), divided into two groups of 150 each, served in the experiment. At the time the experiment was conducted, every *S* had completed one and one-half years of elementary psychology. All were members of one of the three upper classes, the majority being juniors. None had had previous experience with the maze. Over 500 students performed some or all of the experiments, the elimination which determined the final group being based on factors of non-conformity to the above mentioned conditions, or some inequality of factors with respect to the standard methods of procedure. *S*s not normally right-handed were among the group eliminated.

Procedure. The *S*s were divided into groups of two. They worked at three periods separated by intervals of one week. One *S* of every pair performed the experiment with the maze during the first two of the three experimental periods; the other acting in the capacity of experimenter. At the third period the *S* who had acted as experimenter was given the maze to solve. He was of course assisted in his solution by the 'visual guidance' obtained from watching his partner solve the problem at the two earlier periods.[11] The work was done as part of the regular laboratory course.[12] Collection of the data began in 1925 and was completed in 1929.

The procedure on the three experimental days was specifically as follows.

First period. When a given class presented itself for the experiment on the first day, it was divided into two groups, the division being made by two individuals volitionally pairing themselves, with the understanding that such arrangement would continue until the completion of the experiment. Every individual thereby became a prospective member of what will be called hereafter Group I or Group II. The entire group was then informed that Group I was to

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[11]H. A. Carr (*op. cit.*) uses the term 'post-visual guidance' with the recognition that this term involves a memorial phase. Carr's method of supplying this form of guidance, while satisfactory for his study, does not allow an evaluation of that phase in the present research. Inasmuch as the guidance referred to in this study is obtained by visual perception, together with the fact that the relation between perceptual and post-perceptual guidance is not of present interest, the term 'visual guidance' will be used throughout this report.

[12]The method follows that outlined by Professor Karl G. Miller who was responsible for inserting this experiment in the laboratory course. The writer wishes to express his appreciation for the many helpful and timely suggestions made by Professor Miller and for his coöperation in this study.

do a learning problem with a maze without the aid of vision, and that Group II was to do a learning problem with card-sorting.[13] The members of every pair were then allowed to select the type of problem they preferred to do and in most cases the decision was reached by the tossing of a coin. Group I (Maze) was then temporarily dismissed from the room, Group II (Card-Sorting) remaining to be given the instructions by which they would act as experimenters with their partners in tracing the maze. The maze was then shown to Group II and the following instructions read:

"When your partner (*S*) returns to the room and all of Group I have been securely blindfolded, you will be given a maze similar to this one, which you will place before *S*, with the starting point at the right making the first movement of *S* in a direction away from him (cf. Fig. 1). The *S*s will be instructed by me as to the nature of their task. You will place a pencil in *S*'s right hand and guide it in the starting point of the maze. When *S* completes the tracing by reaching the goal, you will take the pencil from the maze and record the time consumed in completing the tracing. One tracing from start to finish will constitute a trial and *S* is to complete 15 trials as his work for the day. You are to keep a time-record of each trial. *S* is at no time to be aided by you, other than at the start and the finish. If *S* gets out of a pathway at any time, replace the pencil at the point it left the path and instruct him to keep on working. Be sure the pencil is held so that no part of the hand comes in contact with the board. The left hand is to be placed along the edge of the board, but at no time must it come in contact with the pattern. Do not tell *S* how many trials he has to do or how many have been completed. Remember you are to give no information and under no circumstance is *S* to see the maze pattern. When he has completed 15 trials place the maze face down in another part of the room."

Questions concerning the instructions were then answered and any doubts concerning the method of procedure were made clear. Group II was then given its method of procedure for the card-sorting to be done when the maze problem was completed and with the members of Group I, in turn, acting as experimenters. These were given directly to the members of Group II to avoid confusion or delay upon the return to the room of Group I and they were instructed to explain the card-sorting procedure to their partners.

Group I was then brought into the room and the following instructions were read.

"You are going to be given a maze problem to do blindfolded, and your partner has been instructed in the required method of guiding your work. Do not ask any questions for none will be answered. The problem is yours to solve by your own adaptation to its nature. A pencil will be placed in your right hand and with it you are to trace your way from the starting point through a series of paths until you come to the end. When you have reached this end or goal, the fact will be made known to you. You are to continue in your efforts from start to finish as the time which is required by you in your tracing will be recorded by your partner. Remember you are to ask no questions and do absolutely what you are told to do by your partner."

The members of Group I were then blindfolded and allowed to adjust themselves comfortably before a table on which the maze was placed. The distance of

[13]The procedure in the card-sorting is omitted inasmuch as it is not a part of the data presented in this study. It was so arranged as to constitute an experimental problem equal in importance to that created by the maze situation. Thus each individual in a pair had his own problem to do.

the individual from the maze was suited to the convenience of *S*. Stop watches and clocks were used as timing devices. If *S* requested a rest period between trials, it was granted, otherwise the trials followed one after the other. During the course of the experiment the writer and some one assisting him were in attendance to see that all conditions of the procedure were adhered to.[14] When the members of Group I had completed the maze problem, Group II worked and completed the task of card-sorting, and the class was dismissed.

Second period. During the second experimental period, which was one week later than the first, Group I again did the maze problem, and Group II continued the card-sorting. Group I was dismissed from the room and the instructions for the work of the day given to Group II.

Under the same conditions as of the previous experimental period each member of Group II was instructed to have his partner do 15 trials of the maze in the following order: 5 trials in the same manner done previously, that is, with the right hand and with the starting point at the right; then 5 trials with the left hand, the maze board remaining in the same position; and finally 5 trials were to be made with the right hand with the board turned at an angle of 180°. This, of course, reversed the direction of all the pathways and made the starting point at the left of the board instead of the right. No indication of these variations were given *S*, his realization of the difference being a part of his adaptation to the situation. A record of the time for each trial was to be noted.

Group I was then called into the room and told that they would again have a maze to do blindfolded. When this was completed by Group I, Group II continued their problem in card-sorting, which likewise had variations as contrasted with the procedure of the first period. As on the first day the maze pattern was not seen at any time by Group I.

Third period. At the start of the third experimental period, which fell one week after the second, the members of Group II who up to this time had been acting as experimenters for the *S*s on the maze and had themselves been working at card-sorting, were informed that they were to do the maze. They were told that it was going to be presented to them under the same conditions as they had given it to the members of Group I at the first period and during the first 5 trials of the second period. This was the first intimation to the members of Group II that they were to act as *S*s on the maze. They were not allowed to see the maze and were immediately blindfolded and given 10 trials. The members of Group II, therefore, had not seen the maze since they had acted as experimenters for the members of Group I, one week previous to this third period. The time of each trial was recorded.

In carrying out the above procedure for the entire experimental group, 4 *S*s were encountered who failed completely to master the maze. Others were given aid when it became obvious that the problem was causing an emotional upset. Finally, certain *S*s varied in several ways from the standard methods, such as missing a given period, not following instructions, etc. In all such cases the results of both individuals in the pair were eliminated from the study. The writer watched very closely for signs in Group II which might indicate an expectancy of doing the maze problem in the third period. If such was suspected, and in some cases it was no more than a suspicion, the results were discarded. The writer fully believes from his observations and reports from various *S*s that a situation

[14]The group were under constant supervision. Not more than 10 *S*s participated in the experiment at one time.

of 'undirected intention' was created for Group II. This belief is based on the following conditions: first, by conducting the experiment in the form of a regular classroom duty; secondly, by giving the members of Group II a particular task (card-sorting) to perform during their periods of observation of the maze; thirdly, by keeping the maze from members of Group I at the end of their second period and thus intimating that they would be called upon to work on it again at the next period; and fourthly, the expressions of surprise which were displayed by members of Group II when informed on the third day as to the nature of their task.

Results

In considering the results of the present study, it should be noted that in any given part a complete mastery of the maze situation was not required of the *S*s, and that the data herein presented pertain only to the early acquisitional stages of the learning curves representative of the conditions investigated. Thus the 'number of trials' made for the several parts is not a function of the individual maze adaptability of the various *S*s; but rather arbitrary criteria chosen following a set of preliminary experiments. This phase of the results, therefore, assumes the rôle of a constant factor in the following analysis.

From a qualitative point of view, several aspects of the responses elicited are of importance. They have their origin either in the mode of attack selected by the *S*s or in certain types of solution to which the maze utilized was particularly adapted. It can be noticed by referring to the diagram of the maze pattern (Fig. 1) that it is possible to reach the goal and include all of the blind alleys in the pathway. This was one method used by a number of *S*s, particularly those in Group I. They would rush through a path until stopped by the cul-de-sac segment and then retrace to the opening. In such cases the error part of the pattern formed the cue which was learned rather than the correct pattern. Some *S*s used a modification of this method. That is, they definitely established certain turns avoiding the cul-de-sac at that point, but at other sections made the blind a part of the path. The fact that the point of the pencil used could only be in contact with one side of the groove allowed for many variations in the methods. Some definitely chose one side of the path as a guide, while others learned to fluctuate the pencil from one side to the other, seeking a break as the cue. In some cases this obtained the desired result, but in others the amount of forward motion injected into this procedure carried them past the correct turn and into a blind. Some rushed through the middle of the path

and disregarded the edges as guides entirely. Finally there were those who established the correct pathway and completed the pattern with none or few errors. All of these variations in method applied to *Ss* of both groups. Thus the matter of errors in a given trial gave little and in some cases no indication of the ability of the individual to solve the situation, namely to trace from the start to the goal. Rather, the qualitative aspect became bettered in terms of the speed with which a given individual adapted himself to a given method, for under those conditions he more readily attained a solution. Hence a quantitative evaluation of errors as a means of presenting a picture of the qualitative aspects did not seem warranted.

The speed of learning seemed to be a more constant means of measurement under the conditions enumerated and for that reason the time required for a given trial is used for quantitative presentation of the results.

This is done in terms of the average for any given trial, for which the mean variation is indicated along with the percentage of variability which it represents. Inasmuch as extreme variability, especially during the initial stages of maze learning, seems to be a function of that type of problem, other measures which might tend to lessen the influence of such cases on the group results are not presented.

First period. Group I had as its problem 15 trials of the maze to do blindfolded with the right hand. Thus the task was one which was 'motor' in its nature. Table I indicates the results which were obtained. The first three columns are the average times in sec. per trial for 3 groups of 50 *Ss*, selected in the order of their completion of the experiment. The fourth column represents the cumulative average per trial for the entire group, the fifth, the mean variation for each trial. The last column shows the percent variation. Curves A and A′ in Fig. 2 show graphically the cumulative averages and mean variations in sec., the solid line represents the former, and the broken line the latter.

The results are typical of the acquisition stages for a problem of this type. The curve shows a very high phase at the start, with a rapid drop and tendency to flatten as the number of trials increases. It is characteristic of the trial-and-error method of approach to the maze situation with adaptation through repetitive performance. The curve shows one inversion, that appearing on the twelfth trial. No

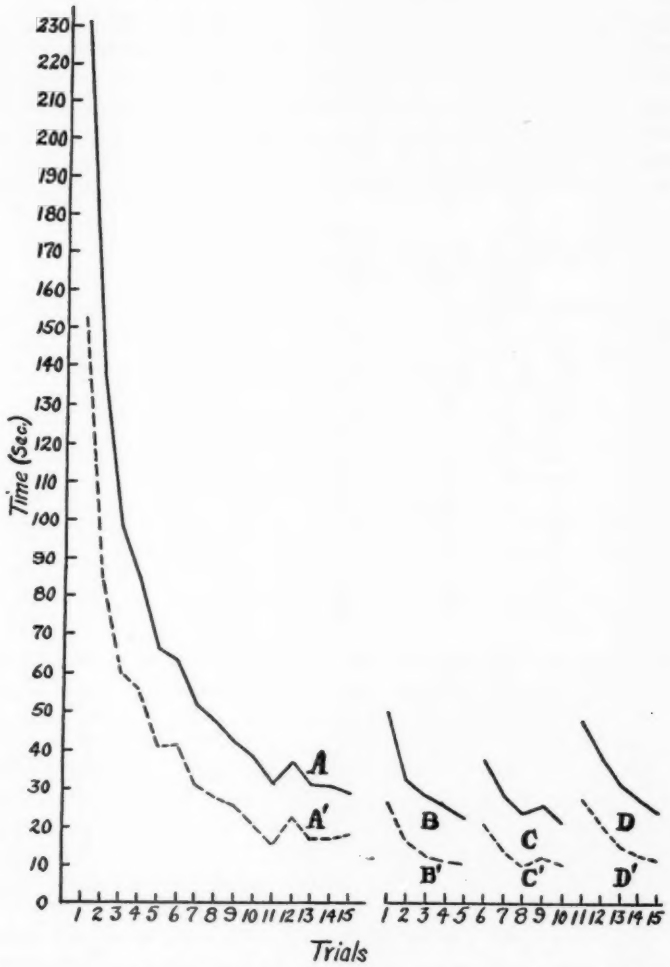

FIG. 2. CURVES SHOWING, FOR EVERY TRIAL IN THE FIRST AND SECOND EXPERIMENTAL PERIODS, THE AVERAGE TIME TAKEN BY THE 150 *Ss* OF GROUP I TO SOLVE THE MAZE-PROBLEM TOGETHER WITH THE M.V. OF THEIR PERFORMANCES

(Solid lines represents average time; broken lines, m.v. The A curves were obtained in the first experimental period and the B, C, and D curves in the second)

significance can be attached to the trial. From Table I it can be noticed that for the first group of 50 *Ss* there is an inversion on the twelfth trial, for the second group three inversions on the sixth, twelfth and fourteenth trials while the third group shows one inversion on the last trial given. A certain amount of temporary unrest was general during the last 5 trials for many of the *Ss*, so that regressions in the learning attitude seem justified at any point during

TABLE I

RESULTS OF GROUP I, FIRST EXPERIMENTAL PERIOD
(Average time of performance, expressed in sec.)

				Total no. *Ss*		
Trials	1st 50 *Ss*	2nd 50 *Ss*	3rd 50 *Ss*	Ave.	M.V.	Percent variation
1	191.0	210.7	293.0	231.5	153.0	66%
2	123.0	135.9	154.1	137.6	85.8	62
3	95.7	91.3	109.2	98.7	60.5	61
4	78.0	82.8	94.7	85.1	55.8	66
5	63.7	60.9	75.8	66.8	41.0	61
6	46.9	69.9	73.3	63.3	41.8	66
7	41.2	61.1	54.8	52.3	31.0	59
8	37.0	55.5	49.2	47.2	27.2	58
9	34.9	50.3	42.1	42.4	25.1	59
10	31.9	49.7	35.1	38.9	21.7	56
11	27.9	37.5	32.0	32.4	15.4	48
12	35.7	43.8	30.5	36.6	23.1	63
13	29.6	36.7	28.7	31.6	16.4	52
14	26.2	39.7	26.1	30.6	16.4	54
15	29.5	32.5	27.5	29.8	17.2	58

these stages. The regression on the sixth trial for the second group of 50 *Ss* seems to indicate that the trial-and-error method is still highly operative. The inclusion of enough cases keeps the cumulative curve positively accelerated at this phase.

An inspection of the results reveals the extreme variability which accompanies human reactions to the maze. During one trial the mean variation drops to 48% but at no other time is it under 50%. During the first, fourth, and sixth trials it reaches 66%. The dotted curve in Fig. 2 indicates the relative constancy of the variability throughout the 15 trials.

Second period. The *Ss* working upon the maze during the second period were those who had the week previous completed 15 trials of the maze with the right hand. Their task for the second period of work was divided into three phases, the results being given in Table II, and graphically by Curves B, B′, C, C′, D, and D′ in Fig. 2. These results are presented in the same form as described for the first experimental period.

The procedure for the first 5 trials of the maze duplicated that of the previous session. An inspection of this part of Table II and Curve B, Fig. 2, indicates the nature of response. The average time for the first trial was 50 sec., that for the fifth trial 23.3 sec., the character of the curve showing a continuous decrease between these two limits, with a rapid drop at the start. The curve itself is indicative of an increasing adaptation to the task.

TABLE II

RESULTS OF GROUP I, SECOND EXPERIMENTAL PERIOD
(Average time of performance expressed in sec.)

				Total no. *Ss*		
Trials	1st 50 *Ss*	2nd 50 *Ss*	3rd 50 *Ss*	Ave.	M.V.	Percent variation
1	48.6	48.7	52.8	50.0	26.5	53
2	32.2	33.8	33.0	33.0	16.0	48
3	26.7	29.4	28.2	28.1	13.2	47
4	23.8	28.2	25.9	25.9	12.4	48
5	20.5	27.9	21.7	23.3	10.5	45
1	31.4	38.4	41.4	37.0	21.2	57
2	26.0	31.3	28.6	28.6	14.1	49
3	20.3	28.9	23.2	24.1	10.0	41
4	22.3	28.3	26.2	25.6	12.8	50
5	20.1	26.7	21.1	22.6	10.5	46
1	41.2	45.2	56.7	47.7	27.1	57
2	41.8	34.8	43.3	39.9	21.7	54
3	26.3	34.7	33.4	31.4	15.2	48
4	22.8	29.4	30.5	27.5	13.4	49
5	22.6	25.2	25.6	24.4	11.5	47

Comparing these results with those of the first period reveals certain points of importance. The start of Curve B is extremely lower than that of Curve A. The actual time difference between the averages of the corresponding first trials amounts to 181.5 sec. The first trial for Curve B compares with that of the seventh trial of Curve A, while the second trial compares with the eleventh trial, disregarding the inversion at the twelfth trial of A. These facts indicate a beneficial result which the previous experience during the first period exerted a week later on the same problem. At the same time the start of Curve B appears to be higher than the point attained in Curve A at the fifteenth trial. Fifty seconds was required for the first trial at the start of the second period, 40% of which is in excess of the time used in completing the last trial during the previous period. This 40% then may be said to represent the amount of forgetting which occurred during the one week interval. A further

inspection of Curve B shows its last 3 trials to be more efficient than any of the trials in Curve A. Hence following the re-adjustment to the situation during the first two trials, the group continued to learn the maze, and indicated by these results that the problem had not been fully learned during the first period.

The mean variations throughout these 5 trials remain high, ranging between 45% and 53%. Despite the extreme variability for this part of the experiment, it is less than that during any part of the first period. While the difference is not great, a tendency toward decrease in variability may be said to be exhibited.

The second phase of the work during this period is represented by the second group of 5 trials inclusive and by Curves C and C′. This followed immediately the first 5 trials and involved one change in the procedure, namely the use of the left hand for tracing instead of the right. Indications of bilateral transfer from hand to hand in one direction and interference with the adaptive reactions to the maze as a result of this modification may be sought.

The previous 5 trials, as shown by Curve B, have indicated that the continuance of acquisition of the original problem had become reëstablished after one week and that the form of its progression was proceeding normally. A comparison between Curves B and C is therefore warranted. Curve C starts lower than B, showing under the conditions a more effective initial adaptation to this situation than that caused by absence from the maze pattern for one week. By the third trial, Curve C is practically the same as the corresponding point on Curve B, and the same is true for the fifth trials. The first trial of C is higher than the final stage of B, this difference amounting to 37% of the time consumed in the initial trial with the left hand. One inversion appears in Curve C on the fourth trial but this is slight and the curve assumes its normal trend immediately. If we compare C and A we note the great difference existing between the initial stages. This amounts to an actual saving of 194.5 sec. between the first two trials. The first trial of C compares in actual time with the tenth trial of A, and as it has been pointed out through the comparison with B, its last trial is lower than that of A. These results then indicate a great degree of transfer from right to left hand and an apparent continuation of acquisition under this condition. The only indication of interference appears in the first two trials of Curve C, but its influence is rapidly overcome in succeeding trials

and it would seem to have a negative value in terms of the acquisition of the maze problem. At the most it proves to be of a temporary nature.

The variability during this phase continues to remain high. For one trial it is 41%, but for the other four ranges from 46% to 57%. This is slightly higher than for the first 5 trials in this period, but the difference does not seem significant. Rather it denotes the constancy of high variation which accompanies human maze response.

The final phase of the second working period involved a solution of the maze with the right hand, but with the board rotated 180°. This followed immediately the five trials with the left hand. Curves D and D′ in Fig. 2 represent this problem. We find the start of Curve D to be slightly lower than that of B but higher than that of C, and progressing in a normal manner to practically the same level as both at the end. Its first trial compares with that of Curve B, while its second trial approaches the first trial of C. The time of the first trial of Curve C is 52% in excess of the last trial for the previous task (Curve C) and 50% in excess of the last trial of Curve B. This might seem to indicate that the interposition of 5 trials with the left hand had little effect on the third phase and that this increase in time points more directly to the change in position of the pattern. In terms of Curve A, the rotation of the pattern caused no marked interference and the carry over of the beneficial results of that period of experience is readily discernible. As in the case of Curve C, representative of a time interval lapse in the problem, and Curve B involving a change of hands, Curve D shows only temporary interference at the start with a rapid readjustment to the continuance of acquisition. It should be noted that all three curves end at practically the same level, which is lower than the final level of Curve A, and that they all give evidence that the learning of the maze is still in its acquisition stages at these points.

For the third phase the variability follows the same trend which marked its display during the previous trials. It again shows itself to be high, its range showing limits of 57% and 47%. The variability then for the entire fifteen trials of this period remains high and fairly constant, with a tendency toward a decrease only during the first five trials, which does not seem significant.

Third period. The work in the third period was done by Group II. These *S*s did 10 trials of the maze under the conditions previously

described as creating for them visual guidance with 'undirected intention.' Table III shows their results in the same manner as heretofore, with one addition. The last column indicates the percentage gain of a given trial for this group as compared with the corresponding trial for the first period of Group I.

TABLE III

RESULTS OF GROUP II, THIRD EXPERIMENTAL PERIOD
(Average time of performance, expressed in sec.)

Trials	1st 50 *Ss*	2nd 50 *Ss*	3rd 50 *Ss*	Total no. *Ss* Ave.	M.V.	Percent variation	% gain per trial
1	73.2	54.6	69.0	65.6	34.6	53	71.7
2	50.7	40.2	48.0	46.3	22.2	48	66.3
3	40.2	36.6	37.1	37.9	16.3	43	61.6
4	32.1	31.9	33.4	32.4	13.3	41	61.9
5	27.9	30.3	29.3	29.1	11.1	38	56.4
6	24.4	27.2	26.0	25.8	9.4	36	59.2
7	27.2	27.6	24.1	26.3	9.8	37	49.7
8	22.9	21.8	21.3	22.0	8.3	38	53.3
9	21.8	20.4	21.0	21.0	7.2	34	50.4
10	19.6	18.4	19.8	19.2	6.8	35	50.6

Curves E and E′ (Fig. 3) show graphically the form of the acquisition stage for Group II, with its corresponding curve of variability. It can be noted that the curve is characteristic in its nature with large initial drops as compared to its final stages. Attention is called to the fact that the average time for the first trial is 65.5 sec. and that within 10 trials the average decreases to 19.7 sec. The significance of this low starting point and final stage will be discussed later.

The variability curve (Curve E′) shows a decrease in relation to the time curve. The decrease ranges from 53% on the first trial to 35% on the tenth trial. (Table III). This also will be discussed later.

Since the third period involves the first 10 trials of Group II on the maze, a comparison of these results should be made with the similar part of the maze problem for Group I. In this respect Group I forms the control group. Such a comparison is shown graphically by the curves in Fig. 4. Curves A and A′ are those for Group I, while E and E′ represent the average times and mean variations for Group II. The effect of the conditions under which Group II did its first trial may be noticed by the actual decrease of 166 sec. which it shows over the corresponding trial for Group I. This amounts to a saving for Group II of 71.7%. By the same means of comparison

for each of the remaining nine corresponding trials, the saving for any given trial may be ascertained. These values are indicated in the last column of Table II. The least amount of saving for any trial is 49.7% on the seventh, while the tenth trial shows 50.6%. By the fifth trial Group II had decreased their actual time value to 29.1 sec., while the lowest time made by Group I during its first period was 29.8 sec., reached on its fifteenth trial. The lowest time made by Group II was 19.2 sec., reached on its tenth and last trial. This is 10.6 sec. faster than the lowest average shown for Group I on its fifteenth trial. The actual time advantage which Group II had for the corresponding tenth trial was 19.7 sec. If we compare the average times for the two groups as indicative of trials saved by Group II, the results would indicate a saving of the first 5 trials used by Group I. This is shown graphically in Fig. 5, by transposing Curve E (Group II) so that it starts at the sixth trial of Curve A (Group II). This transposition also presents graphically the actual superiority which the curve for Group II maintains over the last 10 trials of Group I.

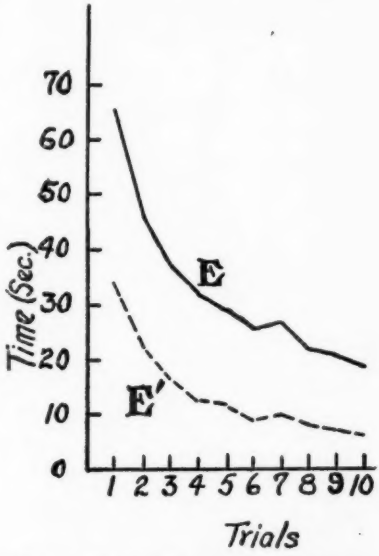

FIG. 3. CURVES SHOWING, FOR EVERY TRIAL IN THE THIRD EXPERIMENTAL PERIOD, THE AVERAGE TIME TAKEN BY THE 150 *Ss* OF GROUP II TO SOLVE THE MAZE-PROBLEM TOGETHER WITH THE M. V. OF THEIR PERFORMANCES
(Solid line represents average time; broken line, m.v.)

The decrease in the percent variability between the two groups is conspicuous, although Group II never shows a mean variation any lower than 34%. Whereas Group I in its first 10 trials shows a range of 56% to 66% variability, the range of a similar period for Group II is 34% to 53%. During the first 5 trials for Group I, the variability is never less than 61%, while all but the first trial of the corresponding series for Group II is under 50%, the fifth trial being 38%.

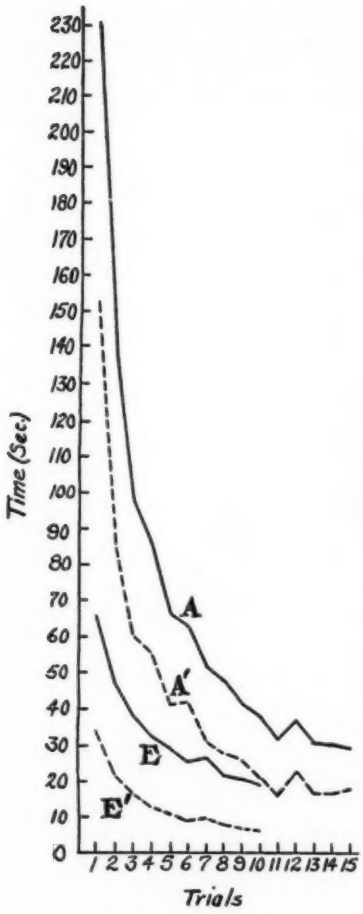

FIG. 4. CURVES COMPARING THE PERFORMANCES OF GROUP II WITH THE FIRST 10 TRIALS OF GROUP I IN THE FIRST EXPERIMENTAL PERIOD

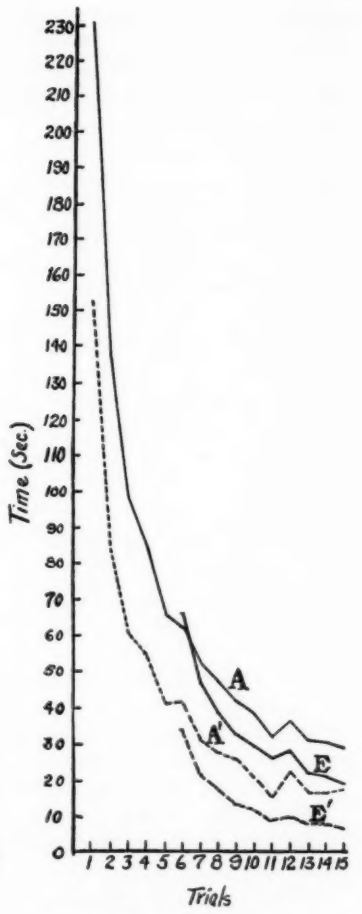

FIG. 5. CURVES COMPARING THE PERFORMANCES OF GROUP II WITH THE LAST 10 TRIALS OF GROUP I IN THE FIRST EXPERIMENTAL PERIOD

(Curves A and A′ show results of Group I; E and E′ the results of Group II. Solid lines represent average time of performance; broken lines the m.v.)

Thus the results show Group II to be superior to Group I during the initial acquisition stages of the two learning curves. This is indicated by the relative percentage of saving for the corresponding trials, the actual speed attained by Group II (superior to even the fifteenth trial of Group I) and by the decrease in the percentage of variability.

The results obtained from Group II lend themselves to a comparison with those obtained during another phase of the work done by Group I. That is, the first 5 trials for Group II may be analyzed with respect to the first 5 trials given Group I at the second period. Group I had at the start of its second session the background for the maze situation resulting from its experience during the 15 trials, completed one week previous. Group II (visual guidance) had witnessed 30 trials done by Group I under the conditions already referred to, their last period of visual observation likewise having occurred a week previous to their actual work with the maze. In Fig. 6 Curves E and E′ are those of Group II, while Curves B and B′ are those of Group I for the 5 trials mentioned above.

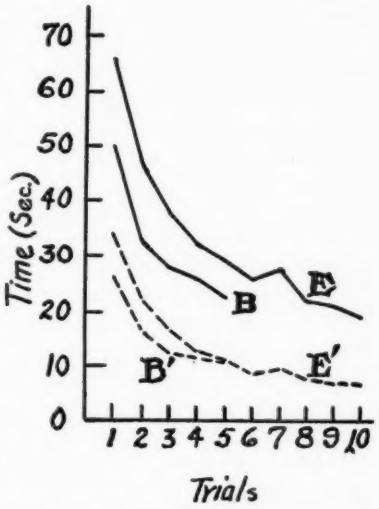

FIG. 6. CURVES COMPARING THE PERFORMANCE OF GROUP II WITH THE FIRST 5 TRIALS OF GROUP I IN THE SECOND EXPERIMENTAL PERIOD

(Curves B and B′ shows results of Group I; E and E′ the results of Group II. Solid lines represent average time of performance, broken lines the m.v.)

From an inspection of the Curves B and E it can be seen that Group I is superior to Group II throughout. For the first trial Group I (Curve B) shows an average time of 15.5 sec. less than that of Group II (Curve E), which equals 23%. For the remaining 4 trials, differences in favor of Group I continue and amount respectively to 28%, 25%, 20%, and 19%. On the basis of this analysis, the 15 trials of actual maze experience gave Group I an advantage in speed of acquisition when this was continued at a later time, as

compared to the visual guidance supplied Group II for their initial attempts at the problem. It should be noted that Group II had actually observed 30 trials of the maze and due to the actual pairing of *S*s their time period of observation of performance doubled that of Group I, not only as a group but individually.

Both groups showed a mean variation of 53% for the first trials. After remaining the same for the second trials (48%) Group I showed a decrease to 45% on its fifth trial, while Group II showed a variability of 38% on the corresponding trial. This would indicate a slight tendency of superiority in this respect for Group II.

Discussion of Results

The interest of the study herein presented centers around the effect of visual guidance on the acquisition stages of a motor learning problem, more specifically that of maze adaptation by humans. In addition, the question of such effect as it might exert itself through a procedure involving what has been termed 'undirected intention' is a part of that interest. Obviously a motor problem such as maze tracing is one in which any cues effecting a more efficient performance must come through actual experience with that situation. In terms of actual sensory experience and discrimination involved, such learning becomes dependent on kinaesthetic and tactual perceptions. The learning process in this case is functioning minus the activity which would accrue from a possibility of visually perceiving the task, and as such is functioning in a manner not most common to its normal operative status. Miles,[15] by his statement that, "vision is the critical supervising partner in nearly all perceptual situations" most aptly gives significance to the condition where that sensory channel is eliminated. Basically there are a number of elements common to both situations, regardless of the 'theory of learning' accepted, and an experimental investigation which allows for a comparison and interpretations suggested by such means, seems justifiable.

The two groups which have been studied and which may be said to represent the two conditions explained above gave objective results which were very different. The one during its acquisition stage displayed a method which was most characteristic of 'motor' learning. They attacked the problem by a series of random movements, some of which brought success, while others did not, and only in terms of a

[15]W. R. Miles, The two-story duplicate maze, *J. Exper. Psychol.*, 10, 1927, 365-377.

number of repetitive performances did they establish a more efficient adaptation. As a result of this method of attack, they attained relatively large increases in efficiency during the early trials, but at the same time their initial acquisition times were far in excess of the visually guided group. At the end of an equal number of trials the difference between the two groups remained large. This was not only true relatively but in actual time required to trace the maze. As we have pointed out under the results, the percentage of time saved by the visually guided group was never less than 49%, while for the first trial it amounted to 71%. The variability between the two groups showed the visually guided group to be more stable in their attack on the problem. The emotional response which was present in Group I, due to the inability to estimate the degree of success and clear knowledge of the nature of the problem, was most noticeably absent in the visually guided group. In all respects which served as criteria of comparison between the two groups, the acquisition of the problem showed the visually guided subjects to be superior.

Such a difference could be brought about by inequality of maze ability among the constituents of each group. Chance also offers a means of obtaining differential results in learning of a maze problem. Heron has indicated how both these factors may function in influencing results.[16] The writer does not believe that these factors account for the difference revealed. Objective dissimilarities in the nature of the problem would be causal factors determining a difference, but again under the conditions of the experiment this interpretation does not seem warranted.

Earlier, the writer has referred to other investigators who used methods of visual guidance and the nature of the results obtained in this study are in general agreement with their findings. The method used here, however, differs noticeably in two ways: first, the reactions from which the results were obtained remained in the form of a normal maze situation in all respects; and secondly, a form of complete visual guidance was supplied, and by this we mean the *S* not only perceived the entire situation but also had the opportunity to observe the type of correct and incorrect responses in operation. As has been noted, to gain these effects the guidance was post-visual in its degree. Under these conditions, then, the validity of the state-

[16]W. T. Heron, Individual differences in ability vs. chance in learning the stylus maze, *Comp. Psychol. Monog.*, 2, 1924, 1-60.

ment that the visual guidance supplied was the most effective cause for the superiority of the second group seems justified.

It is interesting to note one or two conclusions which might be inferred as to the actual difference in the functioning of the learning process responsible for the degree of dissimilarity in the results. For the Gestalt group a situation would seem to have been created with an added 'insight' into the nature of the task. The visual guidance would seem to have allowed for a more intelligent type of learning as contrasted with that dependent on chance, for the configuration involved would be given a greater completeness. Of course in this respect, as the visual guidance was not of a perceptual nature at the time the second group was actually doing the maze, their behavior would not take place with complete 'insight' and so they would be only partially better than the non-guided group. This is what the results of this part of the study show. Hunter concludes that the Gestalt conditions allowing for 'insight' "are fulfilled only with visual stimuli and the result is that insight would thus be limited to visually determined behavior. This in itself leads one to question the hypothesis, for it is highly doubtful that so important a factor as insight is alleged to be would be limited in this manner."[17] Hunter feels that "no causal factor of insight has been demonstrated," and concludes that it is not necessary to set up two kinds of learning as opposed to that which results from "the modification of behavior as a result of repeated stimulation under specified conditions."[18] Another comparison between the two groups in this investigation would tend to show that a higher degree of acquisition of the maze problem can be attained in less time by actual performance with no visual guidance. This would tend to support some of Hunter's conclusions.

When the non-visually guided group performed the maze for 5 trials after a lapse of one week, they showed a more advanced stage of acquisition than did the visually guided group on their initial 5 trials. Both groups were equal in that their last experience with the maze had been one week previous. But the individuals of the non-visual group had worked through but 15 trials and had consumed only one half the time which the visual group had exercised in observing 30 trials. It therefore seems that actual practice with the maze situation had a greater influence on the factors in the learning

[17]W. S. Hunter, *The Foundations of Experimental Psychology*, 1929, 576.
[18]Hunter, *ibid.*, 581.

process than did the visual guidance under the conditions herein presented. If the inference is to be made that the so-called factor of 'insight' by visual guidance made Group II more proficient in a given stage of acquisition, then it must be admitted that the attainment of that same level, by Group I, under the procedure used here warrants an assumption of an 'insight' factor in their method of learning when that level is reached. In this case, of course, this subjective state has been reached without any visual perception of the problem. This would support Hunter's claim that so called intelligent behavior in the learning process is not necessarily limited by visual perception. It would also seem to indicate that the learning process underlying the displays given by both groups were essentially the same kind. The writer believes this to be true.

It seems logical that the acquisition of a maze habit can be made more efficient through familiarity with all of the factors involved in its solution. This familiarity would seem to depend on the degree with which an individual becomes both mentally and physically oriented to the problem it represents. This experiment suggests that visual guidance supplies more quickly, at the start, necessary components of that orientation and therefore the acquisition stages under such conditions exhibit a high degree of objective adaptation. At the same time, it also suggests that part of the orientation comes through actual maze experience, for the curve achieved under such conditions, while relatively more efficient, shows much the same form as that of the non-visually guided. Further, this study tends to indicate that the same degree of orientation may be gained through actual experience in a maze situation in less time. Probably the difference in the actual functioning of the learning process is one which is explained in terms of its 'manner' of operating and not that involving another 'kind' of learning.

The procedure followed in this study made for conditions which have been termed 'undirected intention,' for the visually-guided group. It is *assumed* that during their period of observation they were not doing so with the intent to learn the maze for future experience. As we have stated, this attitude seemed to exist on the part of the *S*s, although a comparative study should be made under which a 'directed intention' attitude to learn is definitely established. In view of the fact that any evidence of a 'directed intention' eliminated an *S* from the group studied and likewise because no objective

factors of a 'directed intention' were a part of the procedure, it is believed that the result of visual guidance may be said to have become effective in those terms. No statement, however, is made as to the degree of influence which this exerted on the results of the study.

The results indicating transfer from right to left hand and lack of interference by rotating the maze board 180° show an agreement with workers who have investigated these same phases under more controlled and specific conditions. Ewert[19] and Bray[20] have experimentally shown the possibility of bilateral transfer. In this study the change of hands caused practically no interference and the group continued to progress in its learning of the maze. The same may be said of the change of position of the board. Hunter found practically no interference due to such a change,[21] a similar conclusion to the one indicated by the results of this study. In addition, the amount of forgetting over a period of one week can be noted by comparing the results of Group I in their first two experimental periods. Presented with the same problem as the previous week, the group showed at the start of this problem a loss of 40% in time, but by the third trial acquisition was proceeding more efficiently than at any trial during the first period. Thus, what forgetting occurred exerted only a temporary influence on the group's tendency to continue to learn. The last three phases of the problem have been discussed but briefly, a more complete interpretation being shown under the results. Their main purpose in the experiment was to furnish further means of comparison with Group II and to serve the purpose of a complete form of visual guidance.

Finally, the high variability which is to be found in problems of the maze type was apparent through all parts of this study. This seems highly indicative of the type of solution which the maze requires and obviously has a high interpretative value with regard to the manner in which the learning process functions in this kind of task.

Conclusions

Summarizing then in a more specific manner the conclusions suggested by this experiment, we might list the following:

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[19]P. H. Ewert, Bilateral transfer in mirror drawing, *Ped. Sem.*, 33, 1926, 235-249.

[20]C. W. Bray, Transfer of learning, *J. Exper. Psychol.*, 11, 1928, 443-467.

[21]W. S. Hunter, Habit interference in the white rat and in human subjects, *J. Comp. Psychol.*, 11, 1922, 29-59.

(1) That visual guidance (post-visual degree) was highly effective in increasing the efficiency for the early acquisition stages of learning in a blindfold cul-de-sac maze (pencil type).

(2) That such results may be found even under conditions of 'undirected intention' on the part of those subjected to visual guidance.

(3) That there is indication that the attainment of a higher degree of efficiency may be reached in less time by actual practice on the maze than that to be effected by guidance.

(4) That under the conditions of the experiment there was interference only of a temporary nature by changing from right to left hand and that re-adjustment to a continuance of acquisition of the maze was rapid.

(5) That a statement similar to (4) may be made regarding a 180° change of position of the entire pattern.

(6) That interference with continued acquisition of the maze amounted to 40% as a result of a one-week interval. This, however, was only temporarily effective.

(7) That a large degree of variability is indicated for all parts of the maze work presented and as such appears to be a function of the type of learning such a problem involves.

SOURCES AND PROCESSES IN THE WRITING OF FICTION

By Warrene Piper, New York City

The following paper contains an account of the genesis of my novel, *Son of John Winteringham*, and an attempt to trace the foundation of the writing habit in my particular instance. Incessant practice in story-telling over a period of twenty years before I undertook to put anything on paper has revealed certain factors and stages in the writing of fiction which seem to be worth the relating.

The actual facts concerning the origin of the book, *Son of John Winteringham*, are as follows. For a good many years I had cultivated the habit of continuing any book that took my fancy; but until quite late in the day no book attained such a hold on my imagination that it furnished an incentive for continued attempts to reproduce it or to take it over. I did have plots that recurred, but they were vague, generic, and not to be traced to any single book as a source. One book, however, eventually established this hold and my mind returned to it again and again in a constant endeavor to carry it on. This was Jane Austen's *Pride and Prejudice;* and by the summary and highhanded processes of the imagination the forced marriage and undesirable brother-in-law of the end of *Pride and Prejudice*, telescoping with a half-conscious recollection of the adoption of Fanny in the beginning of *Mansfield Park*, produced the conviction that there was one further trial reserved for Darcy's mortified pride: that of being forced to adopt the children of Lydia and Wickham. Having made their marriage and his own, everything must be made to flow inevitably from his responsibility for these two facts. That is the simple basis of the book.

In looking at my own book it seems obvious that the gift, if any, is for dialogue and character. Description is almost altogether lacking except in the form of analysis; and action is always felt obscurely as a difficulty. Even so slight an action as crossing a room or a gesture presents a barrier that I can feel as a difficulty to be surmounted. Although I often apprehend the individual peculiarity and emotional significance of movements made by my characters and

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could perhaps reproduce them, I cannot describe them. Therefore when action appears abrupt, violent or ill-prepared and unexpected it is owing to this difficulty, as is also the fact that it sometimes takes place very rapidly and with a character of passion hard to account for unless it is taken as the measure of the resistance overcome.

The method of approach is always through a character of the plot, but not, perhaps, chiefly through any single one of them: although the attempt is to make the principal character so thoroughly known in all his aspects that the act of perceiving him itself defines the perceiver. This boy reveals himself: the others reveal him and in so doing they depict themselves. The other characters are of course far from being a clear undistorting medium; each has his characteristic bias: they make mistakes, they misinterpret, they exaggerate, they miss the point. It can only be hoped that the audience does not do the same. I have tried, however, to make apparent where they are clear-sighted and where they fall short. This method gives the author a somewhat Protean character and, as he never speaks for himself, allows him to speak with a number of individual voices.

There are at least two other peculiarities that obviously depend on the individuality of the author. There are no women among the principal characters. Two are perfunctorily treated. There are two girls but neither is of the first importance. The two chief characters are a man and a boy. There are five other boys in the book. The second peculiarity—one not likely to be obvious to anyone less closely connected with it than the author—is the fact that when everything that has been built upon it has been stripped away there remains a basic feeling, an insistence on the appearance of some uniquely satisfying personality to respond to an urgent need, a preoccupation above all with the story of the lonely uncomforted child. That story is not confined to this book but will in all probability run through everything that may still be written. I apprehended such a situation when I was seven; but I cannot remember that I was either the comforter or the comforted; for, by that time, I had dissociated myself from my story. The result was that, in some way, I was both of them. If anything, as I remember it, I should say that the balance inclined to a possible identification with the comforter (this has a very close connection with the type of my principal character), but with a perfectly clear-cut realization also of the state of the person comforted (a state of mind repeatedly associated with one after another of my secondary characters). This may represent what re-

mains of an earlier period when my identification was reversed and I was more the comforted than the comforter.

My personal history contains elements that seem to show a roughly corresponding trend. I was born a foreigner in the only country that was to be well known to me until after I was twenty-one. I was taught to consider myself an American but educated in every respect as an Australian. We were a small and rather isolated group without family connections or any close friends. My mother was an invalid, ill more especially in the years when I was two and three years old. My father was a busy man not then long in the country; my only sister five years older than myself. I was brought up by an excellent nurse on strictly English and Australian ideas. We had no liberty as American children know it; but we had the best freedom of being by ourselves. I was not an active and energetic child and I became so perfectly convinced, by playing with older children, of the hopelessness of keeping up with any physical performance that all ambition in this direction disappeared at an early age. My mind turned naturally away from what I knew I should not be able to do well and I grew up without any interest in sports or even in dancing. Nevertheless, I believe that I was an unusually happy child, placid, unenterprising, lazy, and easy-going, and always perfectly satisfied so long as I was left by myself. At seven I had discovered that I could tell myself stories.

It was in that year that I was ill-advised enough to admit that I could tell the most magnificent stories in the world. Of course I was called upon to tell them. What was my horror to discover that not only could I *not* tell them but that, being required to tell them as concerning 'a little girl called Jessie,' I could not tell anything except under my own name. (Previously I had not been aware that I was using my own name.) The other children were, as children often are, triumphantly cruel. I had never suffered so much in my life; but I was not capable of giving in. That night I told myself a story under the name of Jessie and since then I have never told a story under my own name or consciously identified myself with my subject. Not only that but, as a child or as a girl, I never—or so rarely that I can recollect them as special instances—imagined *myself* as doing or saying anything. I had no day-dreams, I carried on no imaginary conversations, which were definitely dismissed as a futile proceeding and one that it was incumbent on anyone with self-respect not to indulge in.

But if I could not speak in my own name I could speak in other people's. For a long time it was the 'girl called Jessie;' I do not know definitely when she changed into a boy but, I believe, as soon as I began to read boy's stories. From that time it was never anything but a boy. I read Henty, Strang, and Brereton, and my heroes behaved accordingly. Now as to these heroes, of course the game was that they were not identified with myself; but the connection must have been pretty close and the convention pretty obvious. What was important was that it did provide the convention that could later be turned into something more like the real thing.

I think it would be only accurate to say that this story-telling faculty, common in children, was intermittent in its activity and might, as I grew older, have disappeared under the regular uneventful current of a child's normal experience. As soon as my mother was sufficiently recovered to travel, however, we began to make, every four or five years, trips back to America. I had been twice round the world before I was twelve. In the next year, owing to our removal to another part of the city, I was sent to a new school. These changes had a very definite effect on me as I remember it; the disruption of habit and the readjustment were painful to me and—as always when life was not satisfactory—the story-telling faculty returned. This is, as I think, the second essential point (the dissociation of the idea-of-self being the first). All children may tell themselves stories; but it takes emotional disturbance to cause the faculty to return, and it takes continued discomfort for it to become a mental habit. Our moving was more disturbing than our travelling. It meant a new school. But also it meant, for one reason or another, that I was out of school a whole term. My sister was ill: during these months I was very much alone. I had all the time in the world and nothing to do. I am afraid that my ideal in life has always been to be alone and not to have to do anything. The result was that when I did go to school I was entirely miserable. I had never been good at dealing with new people and here everyone was new. I was entirely out of the habit of playing or doing anything a normal child was doing in my day: I was consequently terrified of anyone my own age, and I was, unfortunately, very much bored by what they considered as something to do. For three months—and I think longer—I began a new story as soon as the school gate closed on me in the afternoon and I lived in a world of my own, refusing to remember that tomorrow must come; refusing to know that I must ever go to school again, telling my

story until the gate closed on me in the morning. It is, as I think, a point worth remarking that there are no habits so tenacious as those bitten in by emotion.

This activity of course diminished as I settled into the ordinary routine of existence; but it persisted to some extent, and meanwhile I had acquired a new form of it. That was simply to retell according to my own fancy anything I read that was either particularly satisfying or in any way unsatisfactory. This rather tended to supersede the other form, but not entirely. The activity was almost always cast in scenes and dialogues, perhaps for the reason that conversation is the easiest thing to remember and a dramatic scene the most satisfying point of any book; but also partly because I had a trick of catching the character's individual voice, and being able to continue him in other situations while retaining his proper character. This recasting became a fixed habit. This activity, parrot-like at first but later almost automatic, evolving with little control according to its own laws, was in the beginning entirely distinct from the other kind. It was, no doubt, excellent training; but the two kinds of story-telling did not overlap. My 'constitutional-story' did not often, if ever, coincide with these supplementary ones. It appeared more rarely perhaps; but when it did it was distinguishable. That is not to say that there was always one story; there were different stories; but they were distinct from the others in not being immediately suggested, although they might absorb characters and situations so suggested. Their distinctive characteristic was in being not so articulate; more, I believe, a projection of states of emotion not always put into words. It is hard for me not to see in these stories the survival of my earlier childish disguised 'identification' states gradually taking over the results of my later practice and the experience I had gained from others' work. All I can definitely say, however, is that they did take over characters and situations without, thereby, being very much improved. They were, I hardly need add, stories of very ordinary types, perfectly undistinguished states of crude emotion. They are the easily recognized staple of popular fiction. At this time I had definitely dismissed all idea of making anything out of them. They were a personal indulgence and a complete secret.

When I was sixteen I left school to go once more to America. It was from this time that, things running uneventfully in the lines of habit, the story-telling faculty dropped to almost its lowest ebb. I continued to practice on other people's characters; but not once or

twice a year on anything of my own. This state of affairs remained in force until, six years later, we left Australia and returned to live permanently in America. No other change had been so complete or so naturally a reversal of everything that had been known to me.

In some ways I dreaded this return to America because I knew that it would be a return to isolation. I looked back on my experience and decided that, although it had been exceedingly pleasant, it had been the period of isolation that had caused many of my former difficulties in going to a new school. I saw that these two situations contained many factors that were alike and I foresaw that something of a similar nature could take place in this second instance. I fully expected the story-telling habit to return. The odd thing was that it did not in the least revive the story-telling faculty. For two years I do not remember seriously even retelling a story. It was after I had apparently settled down in this country that, with leisure and a measurable degree of solitude, I discovered that this faculty had returned to me. This time it was immensely stronger, more automatic, more an involuntary reaction to any sort of stimulus. Now both forms of story-telling came together. The activity started as a continuation of another book and turned into my own.

At this time I know that this fiction retained much of the character of escape. Though I was not badly dissatisfied, I was not yet used to American life, and it is under the aspect of habit that life becomes satisfactory to me. I was to some extent a foreigner in my own country. As witness of this temporary dislocation, the scene of my book was laid neither in America nor in Australia but in England. Reality, actuality was avoided to the extent of causing me to discard both countries that I had lived in, in favor of one that had always been the locale of everything I had preferred in literature. The question of vocabulary, of course, had great influence. Not having an American education I doubted being able to make Americans talk naturally. My ignorance of American life was deep-seated and real; but that was all by the way. There was a disposition to reject America, there was a consciousness that what was native to me I had never found actually existing in Australia. The result was an attachment to literature, to English literature above all, and to one English book in particular. This, as I have said, was *Pride and Prejudice*. I had often tried to continue it and often failed. Among my various efforts was one in which the scene was laid as a direct sequel in time, the first years of the nineteenth century, another in

which it was transferred to America of the present day. I discarded the first because of the research necessary to verify even the smallest irrelevant details, and the imaginative strain of attempting characters free from anachronism of sentiment without falling into mere parody and lack of comprehension was too much for me. The American scene was an even greater failure; but it furnished me with what I consider the essential point of the eventual version, the fact of the principal character being a cripple. That is to say, in addition to solving some of the mechanical difficulties of the plot his quality as a cripple is connected with my own difficulty in presenting action and to that extent brings into play the principle of identification and this accounts for the tenacity and constancy of this character. In his quality as a cripple he may perhaps have taken on a half-conscious symbolic value as standing, not for my character or personality, but for my difficulties. Owing to the then prevailing aspect of escape the scene, therefore, was set in England. Actuality was discarded. Human reality was to take its place if it was possible to attain it.

Human reality, but not realism. When I examined my disposition at the beginning I realized that I was guided by a preference. I knew that my object had never been simply to transcribe things as they generally appear. I was attached to certain aspects and qualities: I wanted these qualities embodied in human form even if it was a lie that these had ever been or could ever be found in any actual human being. I realized that perfectly; they were only the essences that it is possible to distill, for an instant, from hundreds of imperfect lives and to recreate in a more satisfying, a more constant, form.

But this form must be human, it must be real. After discarding actuality I had to cling to every evidence of reality that I could lay my hands on to make a character possible, to make it probable, to make it real; not with any implication that that is what human beings commonly are but that it was what it would be perfectly possible for them to be. Characters therefore were consciously made real in order to give conviction to some desired quality, in an effort to incorporate what was preferred with what was perceived as real.

This dependence on human reality, this interpenetration of perfection and reality with reality predominating, gradually obscured the character of escape. It remained escape only so far as escape enters into the character of catharsis; for whatever it may be for the reader catharsis is the effect of fiction on the writer of it. I cannot say when

it began to take on the aspect of an instrument for understanding and organizing experience, but probably before I was conscious of it.

This is, as I hope, an intelligible account of various steps in the development of the fictitious faculty. With the basis of emotion common to everyone, the natural disposition to project emotion and objectify emotional reactions in the form of a story was encouraged, in my case, by circumstances, and the dissociation of the idea-of-self early freed and strengthened this dramatic and objectifying tendency. The establishment of a habit provided the training and practice necessary to bridge the gap between emotion and words; the return in the form of escape was favored once more by identical circumstances; the idealizing tendency giving value to reality was later subjected to a perception of essential reality, projected under the conception of an imaginative domination of experience by means of the understanding and as a result attained the liberating and stabilizing function of catharsis.

Many of these seemingly unrelated personal facts reappear in the character of the book. There we have my constitutional boy, surrounded by characters each speaking for himself with an individual voice: several variations of the lost child; the English scene; a preference for dialogue; even the difficulty with 'action,' which was probably the unconscious cause of making my chief character a cripple; the prevalence of boys and absence of women, which corresponds with my fictional practice, although it is in inverse ratio to my actual experience; the lack of descriptive faculty, partly, I feel, the result of always reading of what I could not see, and seeing, in my most impressionable years, what had never been adequately described, with a consequent lack of interest in either.

Now as to my chief character the main question is, as it seems to me, the time of its origin. Does anything remain of the unquestionable identification with myself of my first efforts at story-telling? Well! as little as possible; yet, although the character is as far as possible from being my own and in spite of a good deal of deliberate objectivity, I think it obvious that the character derives a certain amount of motive force from this illicit source. There is a disposition to be right and justified that I earnestly combat. There is a tendency to be satisfactory in every situation that I try to counteract by reserving the right to show, in future, the unfavorable 'blanketing' effect of that quality on the more impressionable characters surrounding the main character. Finally, there is the betraying circumstance that this

character is made the center and touchstone of the others. The preponderance of boy characters recalls my early practice, as also the method of presentation, when each character speaks for himself, recalls my borrowed and imitated characters. In my case, therefore, the connection between the character or personality of the author and the fiction produced is much less obvious than that between the circumstances and facts of my existence and the fiction. They are the kindred products of a similar environment.

Let me add a word upon my method of developing and crystallizing my principal character. This character is the result of an immensely complicated series of reactions, preferences and emotional dispositions, superimposed, under and over-lying, shot through and through almost indistinguishably. I have mentioned the possible remains of self-identification and have tried to trace the connection. This self-identification gives vitality; but it cannot provide an individual character because the connection exists at a lower level corresponding to a stage earlier than that at which character is formed. The fact of his being a cripple, arising from mechanical difficulties of the plot, unlocks reserve forces of emotion but it neither characterizes him nor does it identify him with any conception that can form a basis for idealizing activity. The individuality of a character, that quality that can make him appear an actually living being, is the sum-total of a number of theoretical ways of thinking and feeling that have come to hang together, to be constant, habitual, and capable of reproducing themselves in various forms that are always recognized as being 'in character,' *i.e.* as true to their own peculiar constitution. In this way an author may appear to himself to possess a number of secondary characters, formed from the same elements as his own character in real life and acting in fiction characteristically as he himself does in real life, and for the same reason, being compounded of the same elements and put into action in the same manner, but retaining a purely theoretical value because these ways of thinking and feeling exert no effect upon the real-life actions of the author, in whose mind they therefore exist as simply theoretical activity. Authors sometimes say that they have to work on a book only up to a certain point and that after that the characters themselves finish the story. They are evidently trusting to an 'automatic' phase of this activity.

These theoretical ways of feeling and thinking are the result of a number of perceptions and intuitions arising in the form of reactions

to emotional stimuli. They are brought together by some common association and held together by their prevailing emotional tone. These reactions may be provoked by anything; but, in my case, they generally appear in response to words and phrases casually encountered, or in the form of a larger, more generalized, reaction to longer paragraphs, chapters or whole books. This second reaction is less calculable, more unexpected in effect. Intuition or perception seems to go off at a tangent and to have less connection with the facts or ideas that provoked it than with their emotional overtone. Let me take the first reaction—that to words and phrases—as, being more definite, its workings are easier to follow.

Simple characters may be built on a single reaction; but more complex characters consist of almost innumerable separate reactions coalescing in virtue of their common object. These reactions may be of greater or less importance, running all the way from broad generalizations to minute details. Here are some examples of an aggregation of this sort, all referring to my principal and most complex character: Alice Meynell's description of the water in the Roman aqueducts may provide an ideal basis and, personified, activate the individuality of a crippled boy; a chance fling at Elizabeth Bennett may give character to his active will; Santayana's description of Hermes may sum him up; Sainte-Beuve's quotation *à propos de* Montalembert may express his voice; a phrase of Ninon's come to represent his mind, one of Montaigne's express his life, Santayana on *Religion* his spirit at the age of twenty-one, and Marie Lenéru on herself pick out his great faults. Can anyone forsee the result?[1]

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[1]Here are the quotations: "No fairer captive came bound to Rome, none preserved in captivity a more inviolable freedom of heart." (Meynell, *Essays.* This quotation is inaccurate but I reproduce it in the form that did its work on my imagination.) "Elizabeth was always very well able to take care of herself." (Reference in a critical article on Jane Austen. The author I have been unable to identify.) "I have always loved Hermes above all other gods for that charming union which is found in him of youth with experience, alacrity with prudence, modesty with laughter, and a ready tongue with a sound heart." (Santayana, *Soliloquies in England.*) "Chacun a la voix de son esprit." (Sainte-Beuve, *Causeries.* Vol. I., Art. *Montalembert.*) "La joie de l'esprit en marque la force." (Sainte-Beuve, *Causeries,* Vol. I., Art. *Mme. de Sevigné.*) "La vertu est chose plaisante et gaie." (Montaigne, *Essais.*) "A spirit in process of incubation, jealous of its potentialities, averse to definitions and finalities of any kind because it can discern nothing fixed or final. It is adventurous . . . full of rudimentary virtues and *clear fire,* energetic, faithful, *rebellious to experience.*" (Santayana, *Reason in Religion.*) "In the long run it makes us terribly proud always to do without our fellows." (Marie Lenéru, *Autobiography.*) "Having nothing would still be a fine thing." (*Ibid.*)

In explanation of this process, I think it would be safe to say that the decisive effect of these phrases comes from their resuming or defining states of mind that have been vaguely present, waiting for something to precipitate them in crystallized form, states of mind that often represent an enormous amount of uncoördinated work, half-forgotten experiments and plain thinking; or from arousing constant emotional dispositions. Their effectiveness comes from their furnishing a *clou* ready at hand, in summing up what had not been irrevocable before. Let me try to analyze this list I have given.

The first contains the bound-yet-free paradox that has always been one of the most constant and powerful sources of emotional stimulation with me. It must be connected, therefore, with a great number of anterior assertions and reactions; these enrich his character and help to stamp firmly his persistent and unbreakable spirit. The action of this phrase, as I understand it, is as follows. Given the fact of his being crippled and, through his connection with Jane Austen, the fact that as Elizabeth Bennett's nephew he must have wit and independence, these two contrasting situations, together with the swarming and indistinct intuitions resulting from tentative attempts to pose the problem of the relation between these two facts, *are fused and raised to a high level emotionally* by the idea resumed in these words: 'Captive, bound . . . inviolable freedom of heart.' This emotion presents the mind with a distinct apprehension of individuality that persists; and every time it is brought into play reacts in every situation, not in any stereotyped manner, but in every respect *as a person.* The second phrase draws on ideas of self-confidence and independence. These words have had more activating force than any other; they set him going as an entity and keep him going; they form the mainspring of his activity, the state of doing as opposed to the state of being represented by the first phrase. To put the effect of this phrase in quite plain language, when I say to myself: 'Elizabeth was always very well able to look after herself,' Darcy acts instantly, decisively, and recognizably as *himself*, in all his complexity, and the motive force is the emotion aroused by these simple words. The third phrase fixes his character, to some extent, but on lines already laid down. Their usefulness is principally mnemonic; although they arouse ideas that may have given some lasting direction to his character. The connection came in with the 'ready tongue and a sound heart' that he possessed already. The next two I quote merely to show the extent and variety of these reactions. The next (La vertu est chose plaisante

et gaie) calls in an established functioning conviction and embodies a very strong reaction against much modern writing and some of this energy of repudiation passes over to activate the character. The next I give to illustrate the scope of this peculiar faculty. The two last phrases arouse very definite and constant emotional reactions that immediately connect themselves with his position as a cripple, and express strikingly his already formulated sins of pride and refusal to come to terms with life on terms of mediocrity. In all this the only common factor is emotion and I am forced to conclude that, for me, emotion is the source and motive power of mental activity.

There is a peculiar function of the attention that discovers in these phrases and in a hundred others some suggestion or aspect that reveals all sorts of possible characters and sets going any number of associated reflections. This attention is very curious. It is not voluntary, sometimes it is only very reluctantly aroused. One reads whole pages with the feeling 'this ought to start everything,' and not the slightest response. At other times almost anything will do. It is then impossible to read the most ordinary phrases for the swarm of associations, suggestions, crystallizations into character that they arouse. It should perhaps be added that, for what I call 'crystallizations,' purely intellectual suggestions and ideas are insufficient; they are spontaneous reactions and represent the work of the imagination.

Let me recapitulate the various factors and stages that have gone to make up for me the fictional faculty. They correspond roughly to what I regard as the component elements of fiction. There is crude emotion; there is a capacity for projecting emotional states; there is a trained habit of verbalization and of the realizing sense of character; there is the need of escape by means of expression; there is a desire to give a fixed and localized reality to subjective perception; and there is the hope of being able to grasp the discrete elements of reality and to form from them a living conception of experience. The process of automatic action and speech by the characters themselves—also remarked by other writers—and the crystallizations into character by the use of ideas and phrases gleaned from general reading at the time of composition, may be referred more especially to a training in verbalization and in the sense of 'character,' early received and long practiced but reënforced by an idealizing tendency. Emotion I have already identified as the moving force. The projection of emotional states provides the mechanism; and the need of expression, combined with a deep desire to comprehend experience, the animating purpose.

All these factors form an interlocking and interrelated whole in which each plays its part in producing the general result.

The process of plot-formation and character building in its original or early stages has more an aspect of discovery than of invention and the parts discovered by chance, disconnectedly, at very different times, are discovered also very often to fit an already ordered scheme that may not have been consciously present in the mind; other parts are discovered to fit situations that were waiting to be filled by something but unaccompanied by any idea that they were to be filled by *that*, and the agency that causes this discovery is this peculiar form of attention.

This has features that make it strictly comparable to certain kinds of nervous obsession. Just as everything goes to feed the neurotic fear or desire and even a much slighter tension produces something of the same effect (anyone, for instance that has lost a near relation must realize how insistently the name and the fact of death recur at every turn in the course of everyday existence although it normally escapes our notice), so I would suggest that this form of attention is a tension of a similar nature, although it may be set to respond to facts of the widest variety and to have, as its guiding principle, idea, sensation, or intuition indiscriminately and be itself a compound multiplicate tension of the greatest complexity.

One of the simplest and easiest to trace in its formation is the process by which, in the early days of his writing experience, the idea is formed and confirmed in the mind of the writer *that he is able to write*. This, of course, is a very simple tension and consists of the wish to write and the will to believe that he is able to do it. Once this is established everything he reads that can strengthen and confirm this belief is eagerly absorbed and goes to fortify his conviction, every fact that assimilates his activity to the activity of recognized practitioners of the art—of any art or any science—every theory of art that can throw light on the process whereby this activity is produced in him, flows irresistibly into his consciousness and becomes a part of it. This is not a process comparable to the formation of our ordinary ideas and intellectual convictions, the mind does not consciously set out to discover corroborative detail and to verify it, it comes to conclusions. It does not appear to be acting freely of its own volition, details force themselves upon the attention *from the outside* and are verified by *recognition*. The process is one of absorption and impact.

In general absorption is the correct description of the process when the new fact or idea is in harmony with what is already formed in the mind, corresponds to its expectation or conforms to its desires. The new fact or idea is *ingested*, sucked up, and immediately forms a part of the existing whole; impact, on the other hand, is characteristic of anything that directly contradicts or seems to disprove the ruling conception. This impact is sometimes so distinct as to be to some extent physical, the breath is suspended, thought is suspended, in some cases it has a momentary effect on the heart. After this general pause thought is resumed, either immediately concerned with the new aspect presented or returning to it some time later, and a state of intense activity ensues. In general the immediate reaction appears to be concerned with lesser details and the delayed reaction has the more far reaching effect. Delayed reactions occur from twelve to twenty-four hours later. The immediate impression when the reaction is delayed is a decided feeling of depression.

A state of immediate intense activity sometimes approaches overactivity and may degenerate into useless repetition or nervous exhaustion. Sometimes a permanent reaction is established, which may recur either as a simple recapitulation of the original impression or in connection with other impressions and ideas modifying or being modified by them. I cannot too strongly emphasize the involuntary nature of this activity for these are the ideas that arise spontaneously in the mind without previous preparation or conscious endeavour to recall them.

The distinguishing characteristic of this form of activity is that it presents itself in the mind not in the form of idea or hypothesis but in the form of *conviction*. There is a seventeenth century record of a French eccentric, who went to a friend and informed him that the Holy Ghost had told him that he had formerly given him a piece of gold plate of immense value and requested him to return a sum of money equivalent to the value of the piece. It was in vain that the other produced every kind of proof that nothing of the sort had occurred. He could produce no effect for '*The Holy Ghost had said it.*' This is a case of the extreme form of this kind of conviction, which may or may not be founded on fact and justified in its conclusions.

There are apparently certain states of being that are exceptionally favorable to this activity—the functioning of the imagination. The evidence of their existence consists only in a sensation of mental receptiveness, a feeling that anything may be sufficient to awaken an unusual response, a feeling of richness and mental fertility; the mind feels charged. By receptiveness I do not mean receptiveness pure and simple. The mind is not receptive to anything and everything, on the contrary, it is likely to be completely closed to extraneous interests, and often resists the claim on its attention of something it feels to be irrelevant, notwithstanding the fact that after this resistance has been overcome it reacts with unusual richness of comprehension on this unwanted material.

The tendency to reject what comes from the outside seems to be quite characteristic of this condition, the mind feels capable of anything but it is not inclined to go hastily into action, it waits sometimes with anxiety, sometimes actually preferring not to relinquish its favorable state, for something exceptionally suited to its own predisposition. If this condition continues without encountering anything to crystallize its activity it degenerates and exhibits the characteristics of its contrary condition, that of being unfavorable to activity. This is more easily recognized than the other and may arise as degeneration of the first, or as a recognition of the long continued absence of the first or as a species of exhaustion after the imagination has been at work successfully for some time. It is marked above all by *repetition*. Repetition of ideas, sensations or phrases always reveals the fact that attention is flagging, it may end as a slight form of nervous exhaustion accompanied by irritability and super-sensitive behaviour. Hatred of interruption even when there is nothing to interrupt and a fear of forgetting what one had been carrying in the mind are the distinctive characteristics of this state. Whereas previously and when the mind is working freely in a fertile vein no kind of interruption is ever even passingly regarded and ideas and facts are confidently carried in the mind or jotted down if an opportunity occurs. I might add that it is possible to work under both conditions without any appreciable difference in the result, though it might be said that the first state corresponds roughly with the conception of ideas, the second with their working out.

AN ATTEMPT TO ISOLATE THE FACTOR OF 'ATTENTION'

By Howard Easley, Peabody College

A large number of tests have been proposed, and are in current use, for the measurement of attention. The writer sees two serious limitations to their usefulness. In the first place, every one of them is based on assumptions which have not been subjected sufficiently to critical examination; as a consequence, it cannot be said with certainty that attention is really measured by any of them. In the second place, there is no assurance that the attention involved in one test is the same as that involved in another. Correlations between the tests, however high they may be, do not give such assurance, for even a high correlation between two measures of attention may be due to some other common factor and not to attention.

Investigators have held widely different views concerning the nature of attention.[2] Differences here are, however, not so important. Woodrow has pointed out that it is not necessary to agree on the ultimate nature of attention;[3] but holds that it can be measured in terms of its manifestations, as electricity is measured without definite knowledge of its ultimate nature.

Pillsbury says: "So numerous and varied are the ramifications of attention, that we find it defined by competent authorities as a state of muscular contraction and adaptation, as a pure mental activity, as an emotion or feeling, and as a change in the clearness of ideas. Each of the definitions can be justified by the facts, if we put the chief emphasis now upon one phase and now upon another of its varied expressions."[4] Indeed, Geissler[5] and Dallenbach[6] have held that there is a high relationship between such widely different manifestations of attention as degrees of clearness and efficiency in psychophysical acts.

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[1]This study was conducted under the direction of Dr. Joseph Peterson, of Peabody College, to whom the writer is indebted for many valuable suggestions and criticisms.

[2]For an historical review of the different opinions concerning the nature of attention the reader is referred to Geissler's summary (L. R. Geissler, The measurement of attention, this Journal, 20, 1909, 473-502).

[3]Herbert Woodrow, The measurement of attention, *Psychol. Monog.*, 16, 1914, (no. 76), 6.

[4]W. B. Pillsbury, *Attention*, 1908, 1.

[5]L. R. Geissler, *op. cit.*, 502-529.

[6]K. M. Dallenbach, The measurement of attention, this Journal, 24, 1913, 463-509, esp. 507. The measurement of attention in the field of cutaneous sensation, *ibid*, 27, 1916, 445-460.

McComas tested 10 *Ss* using four measures of span of attention, four of concentration, one of mobility of attention, three of ideational types, six of visual perception, four of motor factors in attention, five of auditory factors, and one of memory.[7] He concludes from his results that there are broad types of attention. An examination of his results, however, shows at least ample room for doubt. From his table of 378 correlations between these measures we find that 165 of them are negative. Furthermore, 264 of them are no larger than one standard deviation, 85 others are no larger than two standard deviations, 19 others are less than three standard deviations, and only 10 are larger than three standard deviations. If there were no relation between any of the various measures we should expect 258 of his 378 correlations to be less than one standard deviation, 102 others less than two, 17 others less than three, and one larger than three standard deviations. Comparing these theoretical frequencies with McComas' empirical frequencies, we find little evidence of any relationship. It should be further noted that there is no conspicuous grouping of the higher correlations, which indicates that these broad types are not identified in his results.

McQueen measured 40 *Ss* with five sets of paired tests.[8] One test of each pair was called a concentrative test, and consisted of two operations carried on separately. The other test of each pair was called a diffusive test, and consisted of the same two operations carried on simultaneously. He inferred that the factor of attention was not wholly the same in the two types of tests, since the two tests of the five pairs did not correlate perfectly (these correlations ranged from 0.42 to 0.89). This inference is justified only on the assumption that each test measures perfectly the attention involved in that particular operation, and nothing else. But in a further analysis of the same data, Spearman applied his criterion of tetrad differences and corroborates McQueen's inference, finding no evidence of a group factor of attention.[9] All the intercorrelations between the ten tests were positive, and higher than those of McComas' study. This indicates that the five different types of tests have something in common. In the absence of further correlated measures it is impossible to tell whether these relationships are due to a common factor of attention or to something else which the measures have in common.

Woodrow experimented with 12 *Ss*, using the detractor method of measuring attention in terms of the reduction in speed of response, due to an unfavorable preparatory interval.[10] Simple reactions were used, to sound, light, and touch. Ten of the *Ss* were used with choice-reaction, pressing one key, with one hand, if the light was increased and using the other hand to press another key if the light was decreased. Woodrow assumed that if the factors of attention varied in the individual from one operation to another, then various operations should yield various measures of attention. If, on the other hand, attention is a function

[7]H. C. McComas, Some types of attention, *Psychol. Monog.*, 13, 1911, (no. 55), 9-44.

[8]E. N. McQueen, The distribution of attention, *Brit. J. Psychol., Monog. Suppl.*, 1917, (no. 5), 27-41.

[9]C. Spearman, *The Abilities of Man*, 1927, 268.

[10]Woodrow, The faculty of attention, *J. Exper. Psychol.*, 1, 1916, 293-296.

involving a factor that is constant in the individual, or $A = f(k, c_1, c_2, c_3, \ldots . c_n)$, in which k is a constant factor in the individual, and the cs are the objective conditions peculiar to a given operation; then when the objective conditions are held constant, various functions should give a constant measure of an individual's attention. His results showed a high degree of constancy of the attention involved in the various reactions. The correlations were as follows:

$r_{(\text{sound and light})}$	= 1.00,	age	constant,	= 1.00
$r_{(\text{sound and touch})}$	= 0.99,	"	"	= 0.99
$r_{(\text{touch and light})}$	= 0.98,	"	"	= 0.96
$r_{(\text{choice and av. simple})}$	= 0.99[11]			

Woodrow concludes that "the present investigation has resulted in the experimental demonstration—probably the first such—of a general capacity of attention on the part of the individual."[12] It must be remembered, however, that such a conclusion can only apply to the function of simple reactions. There is no good reason for assuming that if the mode of reaction were widely different from the one used by Woodrow, or if the problem were different or more complex, the measure of an individual's attention would thus remain constant. Woodrow's method does not easily lend itself to such variations.

The problem remains, then, as to whether the current tests of attention measure some special factor which may be called attention. If they do, to what extent does this factor determine the performance of the tests? To what extent is it involved in certain broader mental functions? It was recognized in the beginning that our investigation on the last two points might be limited by the answer to be found to the first problem; and so it turned out.

Method and Procedure

The tests. Pillsbury points out that there are three types of attention-tests: (1) those that would measure the adequacy of attention directly in terms of the accomplishment in some task that involves attention in as high degree as possible; (2) those that make some secondary change in the course of the operation stand as a measure of attention; and (3) those that would make the breaking strain, or the amount of stimulus that is necessary to distract attention, the gauge of the strength of attention.[13]

In this study 5 tests were used: 3 of the first type, and 2 of the second. In view of the mammoth task involved in a general study of this kind, it was deemed best to choose a few representative samples. Whatever inferences are to be made will necessarily be limited by this choice.

(*1*) *Cancellation test.* Pillsbury states that perhaps the most satisfactory method of all those which fall under his first type is the marking of *as* on a page

[11]This r was calculated by the writer from Woodrow's data.

[12]Woodrow, The faculty of attention, *op. cit.*, 315.

[13]Pillsbury, *op. cit.*, 85.

of print.[14] This test, together with many variations of it, is described in great detail by Whipple, whose forms 1 and 2 were used in our study.[15] The first form, beginning *hplg*, contains all the letters of the alphabet, each repeated 50 times in a chance order, but with precaution against the repetition of any letter within two places. The second form, beginning *zycu*, was of similar construction. This type of test was originated by Bourdon, and has been used by a large number of investigators for purposes related to attention. The two forms were given two days apart. Three minutes were given to each form.

(*2*) *Münsterberg test.* The Münsterberg test of attention was the second measure used.[16] This test consists of a page of letters, arranged in such a way that some of the adjacent letter combinations form words, but the majority of them do not. The following is a sample line of the test:

brloeyeldmpykecatholmpiwrbedxaumtinrtmoxkcrytsoobm.

The problem for the *Ss* was to underline all the adjacent letter combinations which formed words. A 3-min. period was given to the test, aside from giving directions. The score was the index of accuracy found by dividing the number of words underlined by the number underlined plus the number omitted.[17]

(*3*) *Woodrow test.* The third measure of attention used was that devised by Woodrow.[18] His method, which he calls the detractor method, consists of taking one measurement of a certain performance under optimum conditions of attention, and another measurement of the same performance under unfavorable conditions of attention. The difference between the two measures is his measure of attention.

The performance used by Woodrow was reaction to a visual stimulus. The optimum conditions of attention consisted of a preparatory or warning interval of exactly 2 sec. before the visual stimulus was given. The unfavorable conditions of attention consisted of long, and irregular preparatory periods, instead of a regular 2-sec. period. By extensive experiments with 3 *Ss* it was found that 2 sec. was the optimum preparatory interval, and that the longer irregular periods were unfavorable to attention. This involves the assumption that reaction-time involves attention to a high degree, although it may not be as pure a measure of attention as the increase in reaction-time due to the unfavorable preparatory interval. By using various intensities of the visual stimulus, he found that the reaction-time varied inversely with the strength of the stimulus. This he assumed to be due to the fact that the strength of attention required to react with a maximum speed varies inversely with the strength of the stimulus. He arranged a series of irregular intervals of warning before the stimulus was to be given, as follows: 4, 20, 17, 7, 15, 18, 9, 19, 8, 12, 18, 6, 10, 16, 11, 5, 17, 10, 4, 15. With these, the reaction-time was found to be longer than with the regular 2-sec. intervals. This increase in reaction-time was found to be greater in the

[14]Pillsbury, *op. cit.*, 87.

[15]G. M. Whipple, *Manual of Mental and Physical Tests*, 1914, 305-310.

[16]H. S. Langfeld and F. H. Allport, *An Elementary Laboratory Course in Psychology*, 1916, 99-100.

[17]Whipple, *op. cit.*, 313.

[18]Woodrow, The measurement of attention, *op. cit.*, 6-15.

case of the stimuli of low intensities than for high. From this he concluded that the increase in reaction-time, due to an unfavorable preparatory interval, is a measure of the attention given in the reaction to the most favorable interval.

In the present experiment every *S* was given one series of 20 visual stimuli, with a preparatory interval of 2 sec. This was followed by two series of 20 stimuli each, with the warning intervals varying from 4-20 sec., according to the order arranged by Woodrow. Another series of 20 stimuli with the regular 2-sec. warning was then given. Then the four series were repeated in the same order, *i.e.* one regular series, two irregular series, and one regular series.

The Dunlap chronoscope, with attachments for visual and auditory stimuli, was used to measure the reaction-times. The *S*s were seated facing the light box, with the eyes about 50 cm. from the opening, which was covered by a translucent ground glass screen. They were given orally the following instructions.

Instructions. This is an experiment in the measurement of attention. You are to react by squeezing this bulb as soon as you see a light in the light box. Exactly 2 sec. before the light comes on, the telegraph sounder, here, will sound, as a sort of warning, or 'get-ready' signal. Then in 2 sec. the light will flash, and you are to squeeze the bulb just as quickly as you can. There will be a series of 20 reactions like that, with the warning period of exactly 2 sec. Then there will be a series of 20 reactions with the warning periods long and irregular, varying from 4 to 20 sec. In that case, when you hear the sound you will not know exactly how long it will be before the light will flash, but try to be ready for it, and squeeze the bulb as quickly as you can. Then we are to have another series of 20 reactions with the long irregular warning periods, and then another series of 20 reactions with the regular 2-sec. warning periods. Then we shall rest for 2 min., and then have the whole test over again. At the end of each series of 20 reactions the sounder will sound 5 or 6 times in rapid succession. This will indicate that you may relax and rest for 30 sec. Before beginning another series, the sounder will sound twice, for you to get ready for the next series.

Before taking any records, the *S*s were given 10 practice experiments: 8 with the regular 2-sec. warning interval, and then one each with the 8- and 12-sec. intervals.

Between the two halves of the experiment, *i.e.* at the end of the second series with 2-sec. intervals, *E* went into the room where the *S* was seated, ascertained that everything was working all right, and indicated that *S* could rest for 2 min.

All the reactions of an *S* were obtained at a single sitting, which lasted approximately 50 min., including the time for giving directions, the practice reactions, and the rest between series. The actual time of experimenting was approximately 40 min. for every *S*.

The chronoscope was operated in a room separated by a hallway from the *S*'s room. The preparatory intervals were measured by a pointer on a large dial of a stop-clock, which measured in fourths of a second. This clock was placed immediately in front of the *E* and ran continuously throughout the experiment with a given *S*. When the pointer on the clock was exactly on one of the second marks, *E* pressed one key which gave the warning signal. During the preparatory interval *E* switched the circuit from the auditory stimulus to the visual stimulus, and at the end of the desired warning interval, as indicated by the pointer on the stop-clock, the key which gave the visual stimulus and at the same time started the marker on the recording dial of the chronoscope was pressed. The pressing of the bulb by the *S* stopped the marker on the chronoscope, thus

giving a measure of the time which elapsed between the giving of the stimulus and *S*'s response. As soon as the figure was read and recorded, another stimulus was given.

The exact duration of the preparatory intervals was to some extent dependent on two reaction-times of *E*, the one involved in giving the warning signal when the pointer on the stop-clock was on a given mark, and the one involved in giving the visual stimulus at the end of the warning period. Before the actual experiment started, *E* practiced on some *S*s not used in the experiment, until the stimuli could be given and the records taken, with the proper warning intervals, without hesitation. On a test of 100 stimuli with the 2-sec. warnings, the actual variations from 2 sec. ranged from zero to 0.11 sec., with a mean of 0.032 sec. The P.E. of this mean was 0.003 sec. This amount of variation is inconsiderable, since Woodrow found that the optimum preparatory interval was only approximately 2 sec. It is obviously of still less importance in the case of the longer intervals.

Since the source of current for the light was four dry cells, there was the possibility that the light would become dimmer as the cells were used. If such were the case to any appreciable extent we should expect the reaction-times to be longer for the later *S*s than for the earlier ones. We should also expect to find a larger difference between the reaction-times with the 2-sec., and the irregular warning periods, since Woodrow found this difference to vary inversely with the strength of the stimulus. In order to check on this point, a record was kept of the day and hour at which every *S* was tested. The mean difference between the reaction-times of the first 34 *S*s tested, with the 2-sec. warning and with the irregular warning, was 79 σ. The mean difference for the last 34 *S*s was 84 σ. The difference of 5 σ may indicate that the last half of the *S*s were reacting under slightly more unfavorable conditions; but the difference is so slight as to be practically negligible. It is also unreliable, since the standard deviation of the difference is 5.2 σ.

(*4*) *Mean variation.* We also used, as one of the measures of attention, the mean of the mean variations in the four series of reaction-times with the 2-sec. preparatory interval. This measure, according to Pillsbury, was suggested by Oehrn: "Oehrn," he says, "suggests that we use the mean variation—the average departure from the average accomplishment—as a measure of the degree of attending. If we make a series of measurements under the same objective conditions, it will be found that no two are exactly alike, and in one series the variation from the average will be large, in another small. Since the objective conditions are alike, the variation must be in subjective factors, most important of which is attention. The mean variation would then indicate the degree of attention, and since it is assumed to be a variation from a maximum, it follows that the mean variation indicates the degree of attention during the series. The results of any experiment whatever, properly interpreted, would furnish a measure of attention."[19]

(*5*) *Reaction-time.* The simple reaction-time has been proposed as a measure of attention, but, as Whipple[20] and Woodrow[21] have pointed out, it is not an

[19]Pillsbury, *op. cit.*, 89.

[20]Whipple, Reaction times as a measure of mental ability, this JOURNAL, 15, 1904, 489-498.

[21]Woodrow, The measurement of attention, *op. cit.*, 7-8.

adequate measure because there are conditions other than attention which affect the reaction. It is recognized, however, that one's maximum speed in reaction requires a high degree of attention. If, then, there is a general factor of attention, the simple reaction-time should be positively correlated with it. The exact size of the correlation should depend on the validity of the other measures of attention and on the relative importance of attention in determining the speed of the reaction. We have used it, in part, as a measure of attention, recognizing its limitation. This limitation should not invalidate it as a means of isolating whatever factor of attention may be present and general throughout the attention tests.

In addition to the scores on the 5 tests of attention, records were obtained from the following tests.

(*6*) *Army alpha.* Performance on the Army Alpha Intelligence Test was obtained from the records of Professor Joseph Peterson, who had, about two months previous to the beginning of this study, examined the students, among whom our *S*s were numbered, with Form 7.

(*7*) *Otis Test.* Records on the Otis Self-Administering Test of Intelligence were also obtained for our *S*s from Professor Peterson. Form A (30 min.) was the variation of the test used by him.

(*8*) *Equation test.* In John C. Peterson's Uniform Intelligence Test every one of the *S*s was given a large sheet of paper both sides of which were filled with arithmetical equations. The problem was to supply the proper symbols to make the equation true. The following are sample equations from the test: 5 5 2 = 8; 6 = 8 4 3. Thirty minutes were given to this test. The *S*s were instructed to draw a line under the last equation finished at the end of the first 15-min. period, when *E* said 'Mark.' This gave two measures on every *S*, for determining the reliability of the test.

(*9*) *Pyle's digit-symbol, symbol-digit test.* In the digit-symbol tests, the *S*s substituted symbols for digits, according to a key at the top of the page. The symbol-digit test was similar, except that digits were substituted for symbols. In each of these, the *S*s were given exactly 5 min. for work.

(*10*) *Whipple's word-building test.* In the word-building test, the *S*s were instructed to write in the spaces provided on the blank for that purpose, all the words they could make from six letters at the top of the blank, using no letter twice in the same word, and using no other letters. The letters for the first form were E A I R L P. For the second form they were B M T A E O. Three minutes were given to each form.

(*11*) *Grades.* Records were taken of the grades made by all the *S*s in a course in general psychology. These consisted of the averages of their scores in 10 weekly quizzes. For the purpose of finding the reliability of this measure, the first and last 5 tests were averaged separately. The average of all 10 of the tests was used for correlation with the other tests used in the experiment.

Subjects. The *S*s used in the experiment were students taking general psychology, during the fall quarter of 1929. They were all undergraduates, most of them sophomores. Three members of the class were not used because of very

defective vision. One other did not take all of the tests. The records of two others were not used, since they failed to follow the direction on the Woodrow test—they reacted to the auditory warning signal, instead of waiting for the light. The records of the remaining 68 members of the class were complete, and were the ones used throughout the experiment.

Results

Table I shows the intercorrelations between the various measures used. The diagonal row in heavy type shows the reliabilities of the various measures, wherever these were found. In the cases of the equation test, the grades, and the Woodrow test, the reliabilities are the correlations between the first and second halves, stepped up by the Spearman-Brown formula, to give the reliability of the whole test. In the case of the digit-symbol test, the reliability is the correlation between the digit-symbol and the symbol-digit forms, stepped up by the Spearman-Brown formula, since the scores on the two were added together for correlation with the other measures. In the word-building and cancellation tests the reliabilities represent the correlations between the two forms of each test, stepped up, since here also the scores on the two forms of each were added together for correlation with the other measures. The reliability of the Münsterberg test represents the correlation between the scores on the even and the odd lines of the test blank, stepped up by the Spearman-Brown formula.

TABLE I

Intercorrelations between All the Measures

a = Army Alpha
b = Otis S.A.
c = Equation
d = Digit-Symbol
e = Word-Building
f = Grades
g = Cancellation
h = Münsterberg
i = Woodrow
j = Reaction-Time
k = Mean Variation

	a	b	c	d	e	f	g	h	i	j	k
b	.68										
c	.50	.64	**.96**								
d	.37	.45	.37	**.76**							
e	.31	.27	.18	.22	**.82**						
f	.60	.60	.49	.21	.19	**.81**					
g	−.08	.01	−.05	.43	.26	−.08	**.86**				
h	.23	.17	.27	.13	.49	.17	.12	**.64**			
i	.10	.00	.14	.12	−.17	.16	−.10	.05	**.72**		
j	.04	−.01	.02	.09	−.12	.06	.08	−.03	−.03	**.94**	
k	.22	.09	.14	.03	−.13	−.07	−.25	.00	.26	.11	**.68**

One of the striking features of this table of correlations is the great number of unreliable and negative correlations between the five measures of attention and the other measures. Since the number of cases was the same throughout the experiment, it is sufficient to state that any one of these correlations, to be reliable, should be as high as 0.32. Only 2 of the correlations involving any one of the measures of attention is as high as 0.32, and 12 of the entire 40 are negative. The relatively high relationship between the word building and the Münsterberg tests is not surprising, even in the absence of other high rs, since in both of these the element of detecting words from letter combinations is relatively prominent. There is nothing quite so obvious in the relationship between digit-symbol and cancellation.

From these correlations it would seem that the 5 measures of attention have very little in common; and, furthermore, that they have very little in common with the other 6 measures. One further test must, however, be applied before we can come to any conclusion about the possibility of isolating the factor of attention by means of the tests used here.

Spearman holds that every mental function is composed of two abilities.[22] One of these, which he calls *g*, is regarded as a general factor, common to all mental functions. The other, which he calls *s*, is a special factor, and unique to every mental function. The relative importance of this *s* also varies from one function to another, a combination of *g* and *s* always accounting completely for any given function. The verification of the theory requires that all the tetrad differences obtained from any table of correlations be equal to zero. Spearman has developed a proof that whenever these tetrad differences do equal zero,[23] there is present in the variables involved only the general factor, *g*, and other factors, specific to the various functions. If, in cases where really different functions are measured, this criterion is not satisfied, it would indicate the presence of group factors, or overlapping between specific factors. If, in the present problem, there is a group factor, attention, we should find the tetrad differences which involve correlations between measures of this factor, varying far from zero. As in all other cases of prediction where there is an imperfect relationship, we should not expect all the tetrad differences to be exactly zero, in order to satisfy the criterion; but

[22]Spearman, *op. cit.*,

[23]Spearman, *op. cit.*, Appendix, i-vii.

rather they should be grouped around zero, within the limits of normal expectancy. Spearman gives a method of calculating the probable error of a distribution of tetrad differences.[24]

The correlations in Table I yielded 990 tetrad differences. Since each of these may be converted into another tetrad difference of equal magnitude, but of opposite sign, we may disregard the signs of the individual differences, and construct a symmetrical frequency polygon, with the 990 tetrad differences on each side of the zero point. This is shown in the accompanying diagram (Fig. 1).

TABLE II

DISTRIBUTIONS OF THE TETRAD DIFFERENCES INVOLVING CORRELATIONS OF THE MEASURES OF ATTENTION

Tetrad Diff.	r_{gh}	r_{gi}	r_{gj}	r_{gk}	r_{hi}	r_{hj}	r_{hk}	r_{ij}	r_{ik}	r_{jk}	Total
0.00	16	16	20	13	32	27	21	35	11	13	194
0.01	10	11	16	5	15	23	18	25	9	16	148
0.02	7	9	13	10	6	15	9	12	6	14	101
0.03	9	11	5	7	8	5	10	0	6	7	68
0.04	6	8	9	5	4	2	3	0	6	7	50
0.05	4	5	7	4	3	0	4	0	3	5	35
0.06	4	8	1	5	2	0	4	0	4	6	34
0.07	6	0	1	1	0	0	0	0	5	3	18
0.08	5	1	0	3	0	0	0	0	2	0	11
0.09	2	1	0	3	1	0	0	0	4	0	11
0.10	1	1	0	0	0	0	1	0	2	0	5
0.11	0	1	0	3	0	0	0	0	3	0	7
0.12	0	0	0	0	1	0	1	0	3	0	5
0.13	0	0	0	1	0	0	0	0	1	0	2
0.14	1	0	0	2	0	0	0	0	1	0	4
0.15	0	0	0	5	0	0	0	0	2	0	7
0.16	0	0	0	3	0	0	1	0	3	0	7
0.17	0	0	0	1	0	0	0	0	1	0	2
0.18	1	0	0	0	0	0	0	0	0	0	1
Med.	.033	.030	.020	.042	.013	.014	.018	.010	.037	.025	.020

The theoretical P.E. of these tetrads is 0.035. The empirical median (of the 990 cases, disregarding signs) is 0.029, indicating that there is no group factor which is very prominently involved in determining the size of the correlations. In spite of this close resemblance to the expected distribution, as far as the median is concerned, there is a slight grouping of tetrads much farther from zero than we should expect. Table III shows the empirical and expected frequencies of all of the tetrad differences. It is quite possible that these large differences could be due to the presence of a factor of attention, however small it may be, or some other factor, not yet isolated. An examination of the tetrad differences which involve cor-

[24]Spearman, *op. cit.*, Appendix, x.

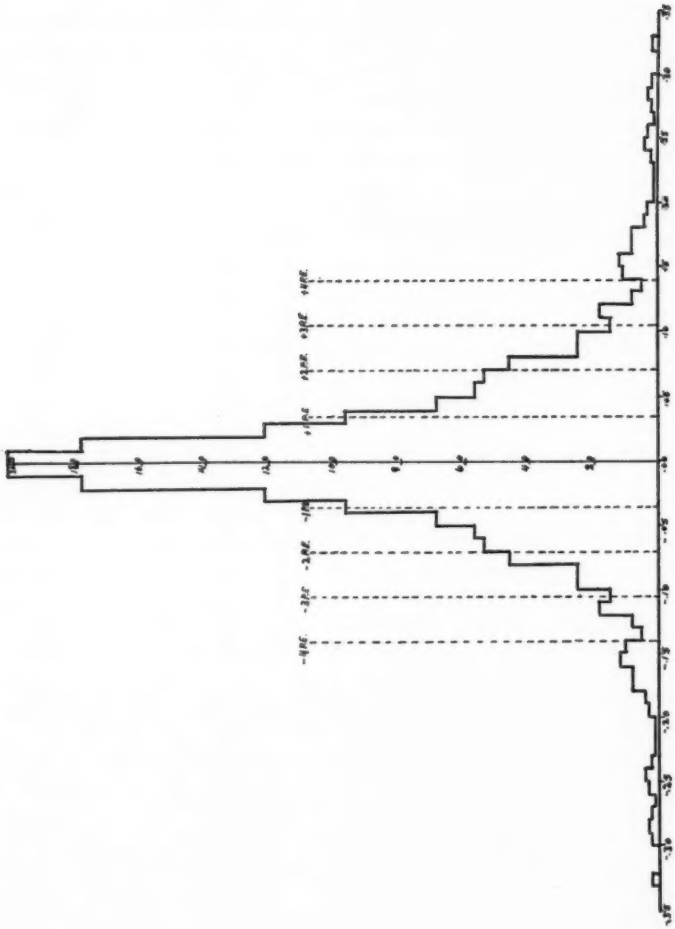

FIG. 1. FREQUENCY POLYGON SHOWING THE DISTRIBUTION OF ALL OF THE TETRAD DIFFERENCES

relations of two of the attention-tests shows that there is no such factor involved in the attention-tests. Table II shows the distribution of the tetrad differences involving correlations of the measures of attention. The empirical median of these tetrads is 0.020, while the theoretical P.E. is 0.035. It will be seen also that the distribution of tetrads involving each of the correlations of attention is well within the limits of normal expectancy. Table IV shows the empirical

TABLE III

EMPIRICAL AND EXPECTED DISTRIBUTIONS OF ALL THE TETRADS

	Empirical	Expected
Below 1 P.E.	550	495
" 2 P.E.	230	319
" 3 P.E.	103	133
" 4 P.E.	39	36
Above 4 P.E.	68	7

TABLE IV

EMPIRICAL AND EXPECTED DISTRIBUTIONS OF TETRADS INVOLVING CORRELATIONS OF MEASURES OF ATTENTION

	Empirical	Expected
Below 1 P.E.	477	360
" 2 P.E.	153	272
" 3 P.E.	43	87
" 4 P.E.	16	36
" 5 P.E.	19	4
Above 5 P.E.	2	1

and expected distributions of tetrads involving correlations of measures of attention, up to five P.E. from zero. If there is any group factor of attention involved in these data, it is not measured by any or all of the tests which specifically propose to measure attention.

Still we have the problem of accounting for the larger of the tetrad differences. Is the common factor of attention involved in some of the other six measures, or is there some other group factor present? It is quite possible that there is a slight group factor present, but if so, it is not involved in the relations between any of the attention-tests. It is also quite possible that, if such a factor exists, it is attention; but if so, it is measured by some of the other tests, and not by any combination of the attention-tests.

Discussion and Conclusions

We have found no evidence that the five attention-tests used in this study really measure any factor which is not measured by any of the other tests. It is possible, theoretically, that they do not measure anything, that the factor of attention still exists, and even that it is measured by some or all of the other tests. There is some slight evidence from our data that a group factor may exist among the other tests, but there is no evidence that it is attention. If it is attention, then the attention-tests are not the best measures for isolating it. It is clear that whatever factors are common to any group of the attention-tests are common to all of them, but it exists in different extents in the various tests. If there is any factor of attention involved in the attention-tests, then all of the eleven tests measure it, and we have merely not succeeded in finding any tests which do not measure it, which would be necessary in order to isolate such a factor. The tetrad differences merely support the conclusion drawn from an inspection of the table of correlations, *i.e.* that the attention-tests have very little in common, and that none of them has much in common with any of the other tests. If attention is common in some degree to all the tests, then the intelligence tests are probably the best measures of it.

It is obvious, from their high reliabilities, that each of the attention-tests measures something. That something may be attention, as has generally been assumed. We have no evidence that it is or is not. If it is, then we can be sure that the attention involved in one function is not the same as that involved in another. This point of view is expressed by Pillsbury when he says "no one of the methods is altogether satisfactory, and no two measure the same capacity."[25] We have one other bit of evidence that the different methods are not measuring the same thing. The Cancellation and Münsterberg Tests are based on the assumption that the functions involved require a high degree of attention. This might as well be assumed for any of the tests used here other than the attention-tests. The Woodrow test is, however, not so dependent on that assumption. It is significant to note that the correlation between the Woodrow test and the reaction-time with the regular 2-sec. interval is -0.03. If reaction-time is a measure of attention, then the Woodrow test is not and

[25]Pillsbury, *op. cit.*, 93.

contrariwise. Which of these two tests is the better measure of the attention involved in that particular operation, we have no objective evidence.

So far as the measures used in this study are concerned, we can draw the following conclusions.

(1) There is little or no relationship among the several measures of 'attention.'

(2) There is little or no relationship between the measures of attention and the other mental functions which we should suppose to require a high degree of attention.

(3) There is no evidence of any group factor of attention involved in any combination of the attention-tests.

(4) If attention is involved in all of the measures to any extent, then the intelligence tests are at least as good measures of it as are the attention-tests.

(5) The direct and indirect methods of measuring attention do not measure the same thing, even in the same function, namely, reaction-time.

STUDIES IN VISUAL IMAGERY

By Henry Bowers, Ottawa Normal School, Canada

In an attempt to determine how an individual's standing in a rating scale of visual imagery is affected by his tendency to exaggerate the clearness or vagueness of his visual images, the writer was led to investigate the possibility of using photographs as experimental materials. The photographic test scale that was as a consequence developed is described in Section I, and the effect of the observer's tendency to exaggerate or to minimize the clearness of his visual images is presented in Section II.

I. Photographs as Experimental Materials in Rating Visual Imagery

The following tests were used in the effort to devise a photographic test scale of visual images.

(*a*) *Test 1.* Objects or scenes were photographed that were familiar to every *O*. A number of prints of different degrees of clarity were made of every picture, ranging from perfect clarity through successive stages of blurring to a complete blank. In the blank only the background, chosen so as to present no detail, was visible. Twenty-four series of these pictures were made; the average number of pictures in every series was 5. Every set was mounted on one page of a booklet, the perfect picture being at the top, the blank at the bottom. Between each two pictures a rectangle, of the same dimensions as each photograph in the particular series, was drawn. A letter was placed beside each picture and each rectangle for the purpose of reference. Alongside or in each rectangle was the instruction, "Imagine that this space contains a photograph midway in clearness between — and —." The dashes stand for the letters attached to the photographs immediately preceding and succeeding the rectangle.

Many of the writer's *O*s had previous experience in introspection and with imagery. A talk was, however, conducted on the subject of visual imagery and practice was given in the forming of images.[1] The booklets and sheets containing the following instructions were then distributed.

Please study the following instructions carefully. This is a device for studying mental pictures. Along the margin of your paper write the numbers 1 to 24, and

*Accepted for publication May 31, 1930.

[1] H. Bowers, Constancy of imaginal content, *J. Educ. Psychol.*, 20, 1929, 295-298.

then: (1) examine the first photograph (A) very closely; (2) now try to form your mental picture of A; (3) examine all the photographs (including A) and decide which one is most like your own mental picture of A; (4) it is quite possible that your mental picture of A is halfway between two of the photographs; then call it by the letter you will notice opposite the proper blank space; (5) note the letter opposite the photograph or blank space where your mental picture is matched best. Write this letter opposite the correct number on your paper (the number is on the back of the folder).

Work slowly, carefully, and conscientiously. In an ordinary examination you are disappointed with a low score and elated with a high one. This is different. If you claim a very clear and distinct mental picture you are not showing special ability, and if you indicate a vague and indistinct one you are not bringing discredit on yourself. Be as cautious and reliable in your choice as you can.

Letters instead of numbers were employed to avoid the suggestion of scores. A haphazard rather than the alphabetic order of letters was used to obviate the appearance of grading. In tabulating, an arbitrary value (0, 1, 2, 3, etc.) was given to each stage. This test required 40 min.

The point may be raised that examination of the blurred photographs in a series would interfere with the original image of A. While recognizing this difficulty, the writer resolved to try out the present technique for two reasons: (a) it affords a scale from zero to perfect imagery; and (b) the use of the 'perfect' picture, A, permits the insertion of a rectangular space (the imaginary photograph) between A and the second picture. The use of the perfect picture, A, as part of the series is held to be justified by the substantial reliability coefficient, +0.92, obtained from only 24 series.

It will be noted that no time limit is set, nor is it suggested that the image be projected on a background. The *O*s were not requested to close their eyes when forming images.

(*b*) *Test 2.* A test was also prepared involving the imagery aroused by isolated words. The *O*s were requested to rate the images evoked by each of 45 words. The following set of instructions was used.

Write the following words one underneath the other: beard, boat, clan, dime, elf, fiend, ghost, grass, hat, hen, jug, path, pew, phrase, plank, porch, pouch, prince, purse, quill, rib, rogue, rug, rust, scar, shrub, sky, slum, snake, swan, tent, thief, toad, tongs, twin, tweed, valve, van, verb, wad, wand, wart, weed, wig, wisp.

Now try to see by means of your mind's eye the object that each of these words represents. Then read through the following descriptions. Decide which description fits your mental picture best.

Would you say: (S) that your mental picture was as clear and distinct as the real object; (F) that your mental picture was almost as clear and distinct as the original object, *i.e.* between S and P; (P) that your mental picture was fairly clear and distinct as compared with the real object, but that it was rather vague and blurred, *i.e.* between F and N; (N) that your mental picture was quite blurred, vague and indistinct as compared with the original object, *i.e.* between P and H; (H) that your mental picture had only the very slightest resemblance to the real object, *i.e.* between N and K; or (K) that you could not form a mental picture of the object?

Now write the letter that stands for the description of your mental picture in front of the word. Work slowly, carefully and conscientiously. Don't think that you are making a high score by claiming a very distinct mental picture, or that you are pulling your mark down by claiming a vague and blurred one. Be as cautious and reliable in your choice as you possibly can.

This test required 15 min.

(*c*) *Test 3.* Seventy-two photographs representing objects or scenes within the experience of every *O* were prepared.[2] These ranged in clarity from a complete blur to perfect distinctness. Every photograph, whether distinct or blurred, was accompanied by a clear photograph of the same object. Both pictures were taken in the same position and at the same distance. Obviously in the case of the perfect blank this precaution was unnecessary. Every pair of photographs was mounted in a booklet, the pictures being side by side. These were distributed with the following instructions.

Please study the following instructions carefully. Along the margin of your paper write the numbers 1 to 72.

Pretend that the second photograph is the picture that you are able to form in your mind of the first one. It doesn't matter today whether it really is or is not your own mental picture; *just suppose that it is.*

Would you say: (S) that the second photograph (your pretended mental picture of the first) was as clear and distinct as the first; (F) that the second photograph (your supposed mental picture of the first) was almost as clear and distinct as the first, *i.e.* between S and P; (P) that the second photograph (your pretended mental picture of the first) was fairly clear and distinct as compared with the first; that it was rather vague and blurred, *i.e.* between F and N; (N) that the second photograph (your supposed mental picture) was quite blurred, vague, and indistinct as compared with the first, *i.e.* between P and H; (H) that the second photograph (your supposed mental picture of the first) had only the very slightest resemblance to the first, *i.e.* between N and K; or (K) that the second photograph (your pretended mental picture of the first) had not even the slightest resemblance to the first, showing that you did not form any mental picture of the first?

Work slowly, carefully and conscientiously. There is no such thing as a high or a low score in this work. Be as cautious and reliable in your choice as you possibly can. Put the *letter* that stands for the correct description opposite the proper number on the margin of your paper. This number is the same as that on the back of your folder. Please be very careful that the letter is opposite the correct number.

Results

A value was attached to every step of the scale. For every pretended image (actual second photograph) the mean of the estimates of the group of 155 *O*s was calculated. That stability was attained in the average estimate for each supposed image in Test 3 is indicated by the almost perfect reliability coefficient +0.99. This figure was obtained by finding the degree of correspondence between the estimates made by two groups of 74 and 77 *O*s from all years in high school.

In using the photographs as a visual imagery test, the *O*s were requested to form their mental image of the perfect picture in a pair and then to examine the other member. If this second picture was the 'same' as their mental image the fact was recorded. No time limit for examination of the original was set nor were the *O*s requested to project the image on a background. It was not suggested that the eyes be closed while the image was being formed. This procedure was followed for the 72 sets. Thus, if an *O* chose a certain photograph, the second

[2]These photographs were those used in the Calibration Test, described in Section II.

TABLE I

ARITHMETIC MEANS, STANDARD DEVIATIONS, AND RELIABILITY COEFFICIENTS

Test	Observers: Age	Observers: No.	Ave.	S.D.dis.	$\sigma_{dis.} \times (1 - r_{11})^{\frac{1}{2}}$	Reliability Coefficient
1	Unselected	181	144	22.5	7	0.90±.01
	1st Yr. H. S.	50	144	18	5	0.92±.02
2	Unselected	151	140	23	7	0.92±.01
	1st Yr. H. S.	40	144	23	7	0.91±.02
3	Unselected	186	2.9	0.5	0.2	0.86±.02
	1st Yr. H. S.	40	2.9	0.5	0.2	0.86±.03
1, 3	Unselected	173	86	11	3	0.94±.01
1, 2, 3	Unselected	132	144	19	4	0.96*±.01

*This coefficient was not derived from the data of the 132 *Os*, but is an approximate value derived from the coefficients 0.92, 0.91, and 0.86 which were obtained for Tests 1, 2, and 3 respectively from the first-year pupils.

member of a pair, as corresponding to his mental image of the first member, we knew the average estimate for that mental picture as rated on a scale of 6 by the whole group of 155 *Os*.

The fairly substantial reliability coefficients of these tests are given in Table I.

It is of interest to note that the standard errors of a score for Tests 1, 2, and 3 are respectively 6/24, 7/45, and 1/5 of an interval. For example, the standard error of a score in Test 2 is 7, or for a single one of the 45 words 7/45, *i.e.* 7/45 of an interval on a scale of 6. This 'interval' is an average and may be looked at as 1/6 of the interval between zero and perfect imagery in the case of Tests 2 and 3, and 1/9 in the case of Test 1.

Tests 1, 2, and 3 were combined into a composite in which every component test was weighted according to its standard deviation and reliability coefficient. These weights were calculated from the formula:[3] $W = (r_{11})^{\frac{1}{2}}/\sigma_{dis.}\ (1 - r_{11})$.

TABLE II

CORRELATIONS BETWEEN TESTS 1, 2, AND 3

Test	Test 2* Unselected	Test 2* 1st Yr. H. S.	Test 3 1st Yr. H. S.	Tests 1, 2, 3 1st Yr. H. S.
1	0.43±.05 (N = 90)	0.47±.08 (N = 39)	0.33±.09 (N = 46)	0.85±.03 (N = 40)
	0.47	0.52	0.45	0.90
2				0.75±.04 (N = 40)
				0.80
3	0.26±.06 (N = 125)	0.17±.09 (N = 43)		0.53±.08 (N = 40)
	0.29	0.18		0.58
1, 3		0.40±.09 (N = 40)		

*Uncorrected scores.

[3] T. L. Kelley, *Interpretation of Educational Measurements*, 1927, 68.

They were for Tests 1, 2, and 3, 0.53, 0.50, and 13.2 respectively. Consequently the composite score (S) for Tests 1, 2, and 3 combined was calculated from the equation $S = 0.4\ S_1 + 0.4\ S_2 + 10\ S_3$, where S_1, S_2, and S_3 are the original scores on Tests 1, 2, and 3. The reliability of this composite is +0.96 and the mean score and standard deviation of a score are 144 and 3.8 respectively.

The coefficients of correlation (corrected for attenuation) between Tests 1, 2, and 3 and the composite test are +0.90, +0.75 and +0.53 respectively. These figures are obtained from 40 first-year high school pupils. The coefficients between

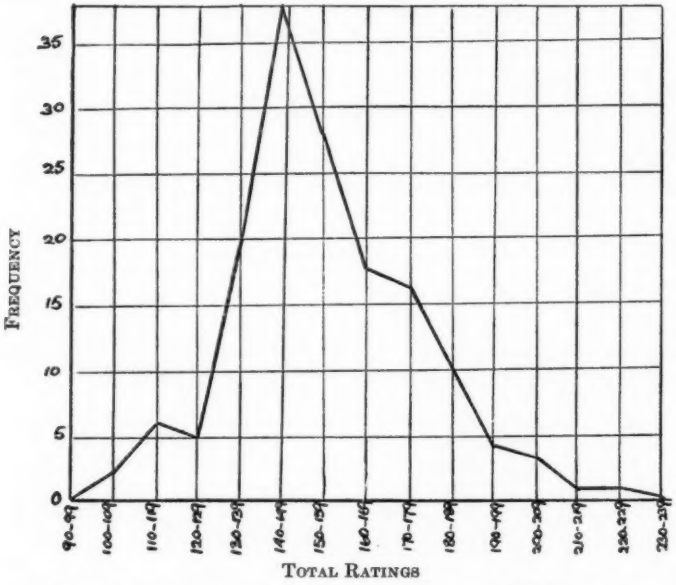

FIG. 1. DISTRIBUTION OF RATINGS ON COMPOSITE OF TESTS 1, 2 AND 3

Tests 1 and 2, and Tests 1 and 3 are +0.52 and +0.45 respectively. A much smaller degree of correspondence is indicated by the coefficient +0.18 obtained between Tests 2 and 3. These figures are reported in Table II.

VALIDITY OF THE TESTS

The establishment of the validity of tests is in general a difficult matter. This difficulty is obviously intensified in the case of a function such as visual imagery. At the outset objection may be raised to the use of blurred and indistinct photographs, however carefully constructed, for the purpose of comparison with visual images. The use of the rectangular blanks in Test 1 was in part suggested by the need of data on this point. It was felt that while there might

be pronounced objection to identifying an image with a particular blurred photograph on account of the shifting, variable nature of the mental image, yet less could be urged against locating the image *between* two pictures. For in this case one's image, a mental product, is compared with another mental product, namely the imagined picture midway in clarity between two given photographs differing in distinctness. An indisputable tendency to use the rectangular spaces rather than the actual pictures would indicate that *Os* found the mental image qualitatively incomparable with the blurred photograph. On the other hand, absence of such a tendency would argue for the validity of blurred photographs as experimental material in the estimation of visual imagery. We shall use as a criterion of validity the ratio: N/T, in which N equals the number of times the image was located in the rectangular space, and T equals the total number of times the image was rated. If there is no tendency to choose the rectangular spaces any more than the pictures this ratio should approximate 1:2. For 182 *Os* the ratio was 1:1.94.

TABLE III

MEAN SCORES, STANDARD DEVIATIONS, AND RELIABILITY COEFFICIENTS OF CERTAIN TESTS PRESUMABLY INVOLVING VISUAL IMAGERY

Test	*Os*	No.	Ave.	S.D.$_{dis.}$	$\sigma_{dis.} \times (1-r_{11})^{\frac{1}{2}}$	Reliability Coefficient
Match Test	1st Yr. H. S.	51	17	10	2.6	0.93±.01
	2nd Yr. H. S.	29	21	9	2.5	0.92±.02
Arithmetic Test	1st Yr. H. S.	50	18	5	2.4	0.77±.04
	2nd Yr. H. S.	30	17	4	2.1	0.73±.07
Counting Test	1st Yr. H. S.	50	100	33	11.9	0.87±.03
Verbal Imagery Test	Unselected	45	120	25.5	3.6	0.98±.01

A test which has been used to measure ability in imaging consists in counting the number of straight lines used to print in upper case letters words such as 'MATE.'[4] For several years the writer has used a test based on this procedure. To make the instructions as simple as possible he requests the *Os* to imagine that the letters are made with matches.

The task is to see the word in the mind's eye and count the number of matches required to construct it. In its present form the 'Match Test' contains 36 elements. There is a gradual increase from 4 to 35 in the number of matches required. One sec. per match is allowed. The statistical data are given in Table III.

The coefficient of correlation between scores on the composite of Tests 1, 2, and 3 and scores on the Match Test is the negligible +0.10 ± .09 for 53 *Os* in the first two years of high school.

A mental arithmetic test was constructed by the writer in a manner apparently calculated to bring into play ability to visualize figures. Advantage was taken of the fact that in speaking numbers such as seven hundred and eleven, five hundred and twenty-four, two hundred and thirty-one, one hundred and seven, the middle

[4]H. O. Rugg, *The Experimental Determination of Mental Discipline in School Studies*, 1916, 66.

digit is not named. Also, there is no specific mention of, say, the first digit in six dozen, the middle digit in two gross, or the first digit in four score. The Arithmetic Test contained 36 roughly graded items. An item was read out and then 10 sec. were allowed for mental computation. The following are typical items: Add the middle digit in seven hundred and eleven to the middle digit in three hundred and twenty-five; multiply the middle digit in two gross[5] by the first digit in seven dozen.

The test was preceded by a brief practice in which it was made certain that everyone was familiar with terms such as 'digit,' 'first digit,' 'score,' 'gross.' The coefficient of correlation between scores on this test and on the composite of Tests 1, 2, and 3 for 53 first and second-year pupils is $-0.04 \pm .09$.

A combination of scores on the Arithmetic and Match Tests was made. Weights were calculated and the r between the two tests was found to be $+0.47 \pm .10$ ($N = 27$). Corrected for attenuation the figure becomes $+0.57$. Negligible correspondence between scores on the combined tests and the composite of Tests 1, 2, and 3 ($r = +0.06 \pm .09$) was found for 53 first- and second-year pupils.

It was considered possible that information on the maximum number of objects that could be counted in a particular visual image might be of use in forming an estimate of the validity of the tests of imagery. For a preliminary 'try-out,' the following Counting Test of two items was given to 82 *Os* of unselected ages.

(1) Form a mental picture of apples spread out on a table. How many can you count in the one picture? Do not form new mental pictures of apples all over the table.
(2) Form a mental picture of a number. How many digits are there in the largest number you can see clearly?"

The r between the number of apples and the number of digits was $+0.39 \pm .09$. Scores on this test gave a correlation of $+0.29 \pm .08$ ($N = 82$) with scores on the composite of Tests 1, 2, and 3. This figure when corrected for attenuation is increased to $+0.48$. Following the 'try-out,' a new Counting Test was constructed. It contained 16 items and had a reliability coefficient of $+0.87 \pm .03$ ($N = 50$) for first-year pupils. The longer test gave scores yielding an r of $+0.40 \pm .10$, with scores on the composite of Tests 1, 2, and 3. This figure corrected for attenuation becomes $+0.44$. Thirty-six first-year pupils were involved.

A test in visual-verbal imagery was also used. This consisted in the rating of images of 30 words and 2 letters on the scale used in Test 2. For 45 *Os* of unselected ages the reliability r of this test was $+0.98 \pm .01$. The degree of correspondence of scores on this test with scores on Tests 1, 2, and 3 combined is indicated by the coefficient $r = +0.82 \pm .01$ ($N = 45$). Corrected for attenuation, $r = +0.85$. The question may be raised as to the contribution of heterogeneity to these two very substantial coefficients. Its effect may be estimated from the following data. The standard deviation of scores on the composite of Tests 1, 2, and 3 for first-year pupils is 17. The standard deviation of scores on the composite of Tests 1, 2, and 3 for 45 *Os* above mentioned is 23. The reliability coefficient of Tests 1, 2, and 3 for first-year pupils is $+0.96$. The reliability coefficient of Tests 1, 2, and 3 for the 45 *Os* is $+0.98$.[6]

[5]For test purposes this is better than the correct form, 'number representing two gross.'

[6]Calculated from the formula: $\sigma/2 = (1-R)^{\frac{1}{2}}/(1-r)^{\frac{1}{2}}$.

It is evident that the probabilities are that figures for a single grade would not differ materially from the two reported. Even if substantial discount is allowed an appreciable degree of correspondence is indicated.

Conclusions

(1) Photographs prepared so that their outline is blurred and detail obscured to varying degrees may be used in the investigation of visual imagery.

(2) A composite visual imagery test of high reliability ($r = +0.96$) has been constructed. This reliability is considered apart from the influence of tendency to over- or under-rate. The composite consists of two tests employing photographs and one requiring the rating of images aroused by words.

(3) There is no correspondence between ability to form images of photographs and ability to count from a mental picture of a word the number of straight lines used in forming that word.

(4) A test in mental arithmetic was constructed so as possibly to bring visual imagery into play. No correspondence was found to exist between scores on this test and those on the tests involving images of photographs.

(5) There is a significant correspondence between ability to form images of photographs and ability to evoke visual images aroused by isolated words.

(6) There is marked correspondence between scores on the composite battery and ability to evoke visual images of words.

(7) Slight correspondence ($r = +0.40$) was obtained between scores on the battery and the total number of items that could be counted in a series of mental images.

(8) Visual imagery as measured by the writer's battery of photograph and word tests is distributed among *Os* representative of high school pupils of all ages approximately according to the probability curve.

II. A Method of Measuring Individual Tendencies to Exaggerate the Clearness of Visual Imagery

The objective measurement of visual imagery appears to involve a contradiction in terms. So apparently hopeless is any attempt to overcome the difficulty of subjectivity that imagery is, in some objective systems of psychology, summarily and contemptuously rejected. While the subjective nature of observation or measurement in this field is admitted, there seems, however, to be at present no basis for conceding (1) that the degree of subjectivity entering into a given estimate is so pronounced that the estimate is vitiated; and (2) that a satisfactory correction for the degree of subjectivity cannot be obtained.

One of the methods employed in the study of visual imagery is that of having the *Os* rate their images on a scale of clearness. Subjectivity resides first in the different shades of interpretation that may be placed on the descriptions of each

stage in the scale, *i.e.* there may be a tendency to exaggeration[7] or minimization in valuing the image[8]; and secondly in carelessness or inaccuracy in matching the image with the description. The second class may be eliminated by use of a sufficiently long test and careful instructions. The former constitutes a different problem. That 'exaggeration' does enter to some extent is certain; that it seriously impairs the validity of the scores is a matter for investigation rather than of opinion.

Problems

Our problem in this section was threefold: (1) To determine if there is a consistent tendency to exaggerate or minimize an estimate of the clarity of visual imagery when using a rating scale. (2) If a consistent tendency exists, to determine the approximate limits of exaggeration or minimization for various percentages of a given population. (3) If the tendency exists, to attempt to apply a correction for it.

Problem 1. Our first problem was to provide a means by which interpretations of the various stages of the rating scale might be compared. The series of 72 pairs of photographs described in the preceding section was used. The *O*s, 155 in number, from all years in high school, rated their 'pretended' images on a 6-step scale.[9] The *O*s were divided into approximately equal groups. The r between mean estimates of each group for every one of the 72 'pretended' images was +0.99. This indicates that a sufficient number of *O*s were employed to give stability to the mean rating of each assumed image.

The reliability of estimate obtained from the total ratings for every *O* on alternate pictures was $+0.89 \pm .01$ ($N = 151$). This shows that under the conditions of this experiment an individual has a consistent tendency to 'exaggerate' in estimating the clarity of visual images when using a rating scale. Two assumptions are involved: (1) that every *O* estimates his own imagery as honestly as he does the

[7]To avoid awkwardness in phrasing, the term 'exaggeration' will be used not only in its ordinary sense but also inclusive of 'zero' exaggeration or identity with the mean, and 'negative' exaggeration or minimization.

[8]G. H. Betts (The distribution and functions of mental imagery, *Columbia Univ. Cont. to Educ.*, 26, 1909, 1-99) found, in his use of a rating scale, that there was a high correlation between scores for visual imagery and those secured in a similar method for auditory and kinaesthetic imagery. We have confirmed these results (cf. *Brit. J. Psychol.*, 21, 1931, 271-82). We suggest that the high correlation might be attributed to a consistent tendency on the part of the *O*s to rate themselves high or low upon the scale. Such a tendency would produce a positive correlation among the various ratings.

[9]For a description of the scale see the preceding paper. It will be noted that this scale is identical with that used in Test 2.

'pretended' images, and (2) that data derived from the study of the supposed images may be applied to real visual images. The writer is convinced that the first assumption is justifiable from his personal knowledge of the *O*s, and from the additional fact that it was empha-

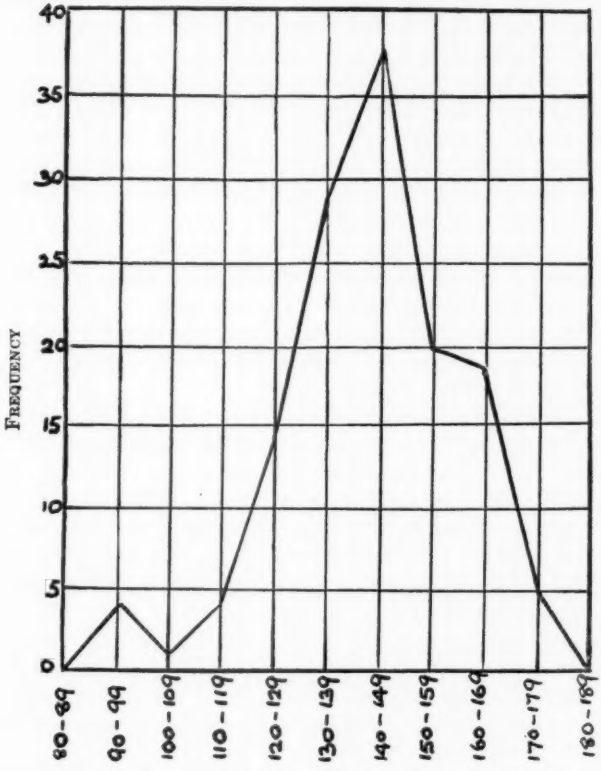

FIG. 2. DISTRIBUTION OF TOTAL ESTIMATES IN THE CALIBRATION TEST

sized that scores on the imagery tests not only had no bearing on school standing but also as far as was known had no connection with intelligence. It cannot be denied that it is easier to rate an actual picture supposed to be an image than an actual visual image. The reliability coefficients for both operations, (+0.89 and +0.92 re-

spectively), obtained from almost identical groups, indicate, however, that too much importance should not be attached to this difference.

Problem 2. We have applied the name 'Calibration Test' to the rating of supposed images by means of the 72 pairs of pictures.[10] Fig. 2 shows graphically the distribution of total estimates on this test. It will be noted, as might be expected, that scores on the test approximate to the normal frequency distribution. Analysis of the frequency distribution of the Calibration Test shows that: 56% of the cases do not 'exaggerate' more than 10/72 of the interval between two stages on a scale of 6; 79% do not 'exaggerate' more than 20/72 of an interval; 89% do not 'exaggerate' more than 30/72 of an interval; and that 95% do not 'exaggerate' more than 40/72 of an interval.

Problem 3. An examination of the mean scores made in Tests 1, 2, and 3 and the battery[11] by *O*s whose mean scores on the Calibration Test fell within certain ranges would indicate which type of test was most likely to yield to correction for 'exaggeration.' These scores are given in Table IV.

It is obvious that scores in Tests 1 and 3 are constant for the 8 ranges. The mean Test 2 score of *O*s in the range 100-149 of the Calibration Test is 138; that for *O*s in the range 160-229 is 144. The S.D. of these means are respectively 1.2 and 2. From these figures may be calculated the S.D. of the difference between the means. This is 2.34. Dividing the difference between the means by the S.D. of this difference we obtain 2.56, a figure barely justifying the hypothesis of a fundamental difference.

In view of the fact that the rating scales used in Test 2 and the Calibration Test are identical, and that actual pictures rather than verbal descriptions are used in Tests 1 and 3 (thereby reducing to some extent the subjective element) it would seem natural to find

[10]This name was used because it was hoped that this technique would make possible direct comparison of individual estimates of imagery by correcting for the tendency to 'exaggeration.'

It will be recalled that in using the 72 pairs of photographs for the estimation of visual imagery (Test 3 in the writer's battery), every *O* selected those blurred photographs which corresponded most closely with his mental image of the 'perfect' picture.[6] Since all of the 72 blurred pictures were rated on a scale of 6 (Calibration Test), a very stable mean rating for each was available. Thus it was possible to assign 'scores' to each picture selected in Test 3.

[11]The combining of Tests 1, 2, and 3 into a battery is described in the preceding section.

TABLE IV

MEAN SCORES ON TESTS 1, 2, AND 3 FOR CERTAIN INTERVALS ON THE RANGE OF SCORES IN THE CALIBRATION TEST

Test	Mean	σdis.	Mean Scores on Tests 1, 2, and 3 for the following Ranges in the Calibration Test							
			100-129	130-139	140-149	150-159	160-169	170-179	180-189	190-229
Calibration	155	21								
1	144	22.5	146 (N = 13)	144 (N = 18)	143 (N = 35)	143 (N = 26)	141 (N = 17)	143 (N = 15)	145 (N = 8)	147 (N = 8)
2	140	23	133 (N = 12)	135 (N = 19)	141 (N = 37)	138 (N = 27)	145 (N = 17)	136 (N = 15)	147 (N = 9)	153 (N = 8)
3	2.9	0.5	3.2 (N = 12)	3.1 (N = 20)	2.8 (N = 37)	3.0 (N = 26)	2.8 (N = 17)	2.9 (N = 15)	2.8 (N = 10)	3.0 (N = 7)
1, 2, 3	144	19	143 (N = 11)	144 (N = 19)	141 (N = 35)	143 (N = 23)	143 (N = 15)	141 (N = 15)	145 (N = 8)	149 (N = 7)

closer connection between scores on the Calibration Test and Test 2 and between those on the Calibration Test and Tests 1 and 3. Slight support for this is afforded by Table IV. Accordingly, the coefficients of correlation between 'corrected' Test 2, and Test 1 and Test 3 scores were compared with those between 'uncorrected' Test 2 scores and Test 1 and 3 scores. Two methods of 'correction' were used. In Method A every Test 2 score was divided by the score of the same *O*

TABLE V

CORRELATION BETWEEN TESTS 1, 2, AND 3 AND THE CALIBRATION

Test	Test 1	Test 2* Unselected	Test 2* First Year Pupils	Test 2† (Scores corrected by Method A) Unselected	Test 2† (Scores corrected by Method A) First Year Pupils	Test 2‡ (Scores corrected by Method B) Unselected
1		0.43±.05 (N=90) 0.47	0.47±.08 (N=39) 0.52	0.36±.06 (N=89) 0.40	0.36±.10 (N=39) 0.40	0.38±.06 (N=85) 0.42
1, 3			0.40±.09 (N=40)		0.29±.10 (N=40) 0.32	
3		0.26±.06 (N=125) 0.29	0.17±.09 (N=43) 0.18		0.09±.10 (N=43) 0.10	0.29±.05 (N=125) 0.32
Calibration	0.04±.06 (N=89)	0.09±.07 (N=90) 0.10				

* Uncorrected scores. † Scores corrected by Method A.
‡ Scores corrected by Method B.

on the Calibration Test. In Method B the mean rating given to each supposed image in the Calibration Test by the group of 155 *O*s was taken as the true value. From these values it was possible to obtain for any *O* what his rating of 5, 4, 3, 2, 1, or 0 was worth. Thus, if he estimated 10 pictures at 4, and if the total of the mean ratings for these pictures was 38, his 4 was considered to be worth only 3.8. This tedious process was carried out for each of the 6 ratings in the case of each *O*.

Comparison of the coefficients listed in Table V shows that the only effect of 'correction' is to reduce slightly the correspondence between scores in Tests 1 and 2.

Conclusions

(1) There is a consistent tendency to exaggerate or minimize an estimate of the clarity of visual imagery when using a rating scale.

(2) There is evidence that the influence of subjectivity in rendering ratings obtained in connection with visual imagery incomparable is not of serious dimensions in the majority of cases.

(3) There is slight evidence that scores derived from the ratings of images aroused by words are more influenced by individual tendencies to over- or under-rate the clarity of imagery than those obtained from the use of photographs. An attempt to correct these scores by means of a calibration test was unsuccessful.

BLOCKING: A NEW PRINCIPLE OF MENTAL FATIGUE

By Arthur G. Bills, University of Chicago

The phenomenon investigated in the following series of experiments is that of 'blocking' in mental work. Incidentally, the study has uncovered a new principle of mental fatigue. The term 'block' will be more rigidly defined later, but briefly it refers to those periods, experienced by mental workers, when they seem unable to respond, and cannot, even by an effort, continue until a short time has elapsed. Blocking has been frequently observed by students of mental work, but has not heretofore been systematically studied to determine (a) what laws govern the occurrence of these blocks, (b) what factors determine their frequency, (c) the relation of blocking to fatigue and the general decrement, (d) its relation to the principle of refractory phase, and (e) the relation of blocking to the occurrence of errors, and to the amount of previous practice in the task.

Several considerations make an understanding of the phenomenon of blocking important. (1) It has been suggested, with good reason, that the failure to find large and rapid decrement in mental work, such as is found in muscular work, is due to the fact that the subject gets frequent short rests which give sufficient opportunity for recovery from accumulated fatigue, and thus cover up or stave off decrement. (2) The fact of a more or less rhythmic fluctuation in the degree and direction of attention has long been recognized on the subjective side, and it is reasonable to suppose that some corresponding form of fluctuation in performance level would be found as soon as a sufficiently refined technique should be devised for investigating it. (3) A consideration of refractory period in nerve operations, especially the recent findings of Forbes[1] pointing to the existence of a cumulative refractory period, of greater length than the simple phase, suggest that a corresponding recurrent gap in the mental activity ought to occur. (4) Granted the existence of such recurrent periods of lowered

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[1]A. Forbes and L. H. Rice, Fatigue in peripheral nerve, *Amer. J. Physiol.*, 90, 1929, 119-145.

mental functioning it would be natural to expect that they would have an important bearing on the occurrence of errors, and would yield important information about the cause of errors.

Tasks. Some preliminary investigations tended to show that blocks are more likely to occur in homogeneous tasks, and those having a high degree of continuity. Moreover, since the length of a block is not great, they would show up best in tasks in which the responses come frequently, so that a gap in the flow of responses would result from each block. In fact, if the responses came too infrequently, blocking would fail to show up. The same would be true if the units of work chosen were too large, since one or more blocks could easily be swallowed up within one unit. Hence in the selection of tasks, it was necessary to get as wide and representative a set of tasks as possible, involving several different mental functions, and at the same time have them homogeneous, continuous, and with frequent easily recordable responses. The following five forms of mental work were selected: (1) alternate addition and subtraction; (2) reversible perspective; (3) color naming; (4) opposites; and (5) substitution.

Apparatus and procedure. The study involved a year's experimentation, in which 50 *Ss*, all advanced students, took part. The general procedure was nearly identical in all the experiments, so that one description will suffice. *S* was seated at one table with his back toward *E* and the apparatus. Before him was displayed the stimulus material, to which he responded orally as rapidly as he could. The directions were presented to him on a typed card to insure uniformity. In every case they directed him to work at maximum speed consistent with reasonable accuracy. *E* sat at another table on which the recording apparatus was mounted. In front of him was a contact key which he pressed every time *S* responded, at the instant the response was made. Another person, an assistant, sitting at the same table, operated another contact key, to record the *S*'s errors. He was supplied with a correct response sheet, so that he could detect errors no matter how fast *S* responded. The two contact keys were connected in circuit with a kymographic recording device consisting of a Zimmerman adjustable speed kymograph, and a continuously unrolling record sheet of cross section paper, stretched between two drums. The markers employed were fountain pens filled with skrip, operated by electro-magnets. Three such pens were mounted side by side, one recording responses, one errors, and one recording time in sec. or 3-sec. intervals, being in circuit with a Bowditch and Balzar contact clock. The paper traveled at the rate of about 6 in. per min. In this way, *E* could tell from reading the record exactly where the errors occurred with relation to the blocks.

A block was arbitrarily defined, for the sake of uniformity, as "a pause in the responses equivalent to the time of two or more average responses." By defining the block in this way, instead of by a constant time unit, it was possible to adjust the criterion relative to the speed of the *S* or the particular period. Thus, for an *S* who responded at a rate of 60 per sec., a block could be as short as 2 sec., but for a person whose rate was 30 per sec., a block must be at least 4 sec. long.

The length of the work periods, and the number of day's records obtained from every *S* differed with the different experiments, and will be discussed in connection with them.

The following problems were studied in this investigation. (1) The frequency and length of the blocks, both absolutely and relatively to the frequency of the responses. (2) The rhythmicality of the blocking, *i.e.* the constancy in number and spacing of blocks from minute to minute. Also the tendency for bunching of responses to occur between blocks. (3) The increase or decrease in frequency and length of blocks during the work period. (4) The increase or decrease in frequency and length as a result of practice. (5) The relation of errors to blocks. (6) Individual differences in speed of response as related to frequency and length of blocks.

Experiment I. Addition and Subtraction

The mental work employed in Experiment I consisted in the alternate addition and subtraction of the number 3 from each of a

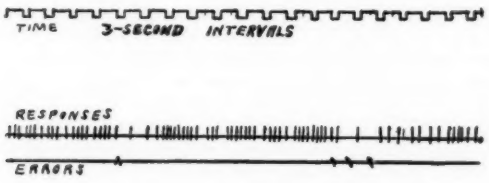

Fig. 1. A Sample of the Kymographic Record (From Experiment I)

long list of digits (between 4 and 9) arranged in continuous rows—40 digits to the row and 24 rows to the page. One sheet therefore contained over 10 min. continuous work.

Ten *S*s took part in the experiment. Every *S* worked for 7 min. continuously, every day for 8 days. Errors were recorded only in the case of 2 *S*s. Fig. 1 shows a section of a kymographic record, displaying the three simultaneous lines. The top line is the time line, the next is the response line and the bottom line shows the position of the errors with relation to the blocks.

The individual records were analysed to determine the number of responses per min., the number of blocks per min., and the average length of the blocks, in successive minutes on every one of the 8 days.

The average results are brought together in Tables I and II. Table I shows the average results by minutes for the average work day, and hence constitutes a work curve. Table II shows the results for an average minute on every one of the 8 days, and hence is a practice curve.

TABLE I

SHOWING EFFECT OF FATIGUE ON RESPONSES AND ON FREQUENCY AND LENGTH OF BLOCKS

(Every value the average of 80 cases)

Addition-Subtraction	Successive minutes of work						
	1	2	3	4	5	6	7
No. of responses per min.	61.0	57.0	56.7	55.7	55.2	55.0	55.8
No. blocks per min.	2.9	3.4	3.7	3.8	4.2	3.9	4.2
Av. length blocks (in sec.)	1.9	2.2	2.3	2.4	2.5	2.9	2.7

TABLE II

SHOWING EFFECT OF PRACTICE ON RESPONSES AND ON FREQUENCY AND LENGTH OF BLOCKS

(Every value the average of 80 cases)

Addition-Subtraction	Successive days of practice							
	1	2	3	4	5	6	7	8
No. responses per min.	36.1	45.7	51.7	58.1	60.4	63.0	66.7	71.0
No. blocks per min.	4.6	4.0	3.3	3.8	3.8	3.5	3.5	3.7
Av. length blocks (in sec.)	3.6	2.9	2.5	2.1	2.1	2.2	1.8	1.8

An examination of Table I shows (a) a slight decrement as the work proceeds in number of responses per min., (b) a proportionally much greater increase in number of blocks per min., equivalent to about 50%, and (c) a 50% increase in the length of the blocks.

It is evident, then, that the frequency and length of blocks forms a much more sensitive criterion of the onset of fatigue than the frequency of responses. In fact it may very well be that the greater amount of rest time resulting from the enforced pauses which these blocks indicate tends to stave off the fatigue and cover up the decrement which would otherwise appear in loss of speed.

The effect of practice is plainly shown in Table II in all three variables, but its effect on blocking is most striking. It can be seen (a) that the increase in responses per min. during the eight days was about 100%, (b) that practice caused a decrease in frequency of blocks amounting to 20%, and (c) that it reduced the length of the blocks 50%.

Perhaps the most striking result of Experiment I is the evidence of rhythmical blocking. For all *S*s under all conditions of practice

and fatigue this rhythm tends to strike an average rate of one block every 17 sec. Individual differences are great, some *S*s block every 10 sec. and some every 30 sec. One *S*, a woman, would sometimes go for 2-3 min., without a block. The phenomenon of rhythmicality is clearly shown in the photographed records appearing in Figs. 1, 4, and 5.

Experiment II. Reversible Perspective

Experiment II differed from the preceding mainly in the type of mental work used. It was intended to select an activity in which control of set by voluntary effort was the main feature. The voluntary control of reversible perspective in ambiguous figures offers such an

TABLE III

Showing Effect of Fatigue on Responses and on Frequency and Length of Blocks

(Every value the average of 80 cases)

Reversible Perspective	Successive minutes of work						
	1	2	3	4	5	6	7
No. responses per min.	72.0	72.0	67.5	63.0	60.3	60.0	60.7
No. blocks per min.	3.4	3.9	4.3	4.5	4.8	4.8	4.6
Av. length blocks (in sec.)	2.8	2.6	2.7	2.8	3.0	3.0	2.9

activity. To a person gazing at such a figure, the reversals will take place involuntarily if time enough is given. As here used, however, the activity consisted in making the reversals occur as often as possible. The figure used was the familiar pile of blocks, which may be perceived in two different solid forms depending upon whether the *S* interprets it as seen from above or below. *S* was presented with a card on which the pile of cubes was drawn, and a set of directions as to how to proceed. He was instructed to try to cause the reversals to occur as rapidly as possible and to say 'Now' every time he achieved a reversal.

In other respects, the arrangement of the apparatus and of the conditions of the experiments was identical with Experiment I. Records for 8 days were secured from every *S;* every record representing 7 min. continuous work. The same *S*s were used, consequently it is possible to compare directly the results of the two experiments. Table III gives the average number of responses by minutes, the average number of blocks, and average length of blocks in every minute for the average working day.

There is a 17% decrement in responses, a 35% increase in frequency of blocks, and, comparing the first and the average of the last three minutes, a 7% increase in the length of the block. These results are similar to those of Experiment I.

An examination of the results of the 8-day practice shows a tendency similar to that in Experiment I (cf. Table II). There is an increase of number of responses per min. from 57.3 to 72.0 or about 26%. The average number of blocks drops from 4.9 to 3.7 per min. or 24%. Length of blocks undergoes no marked reduction, dropping from 3.1 to 3.0 sec., or a little over 3%. The averages are quite reliable, being based on 280 measures each.

Experiment III. Color-Naming

Color-naming was chosen for Experiment III, because it is assumed to involve speed of apprehension, and hence to be a good test of perception. But its chief advantage is its homogeneity as a task. Six units appear repeatedly in all possible orders: red, yellow, green, blue, white, black. It therefore offers a good opportunity to test the hypothesis that the blocking is a refractory phase phenomenon, resulting from the continuous exercise of a limited number of homogeneous neural elements, in rapid succession. The color naming sheets used were the standard Woodworth-Wells type.

The chief differences between the arrangement of this experiment and the preceding ones are these: (1) a new group of *S*s was used, 21 in number, all naïve with respect to the experiment; (2) the work period was lengthened to 10 min.; (3) every *S* reported only twice, once in a preliminary practice series, and once in the regular working period. The increased number of *S*s made it possible to compute individual differences in blocking and to correlate the differences in performance with differences in speed with some assurance of the reliability of the coefficients obtained.

Table IV shows the average work curve for frequency of responses and frequency and length of blocks.

These group results show the same tendencies as those of the preceding studies. There is a 33% increase in number of blocks per min., and an 18% increase in their average length. The same rhythmical regularity of recurrence of blocks is present in these records. And the average rhythm is practically the same as in the other cases, one block every 17 seconds. In fact this constant average rhythm in

TABLE IV

SHOWING EFFECT OF FATIGUE ON RESPONSES AND ON FREQUENCY AND LENGTH OF BLOCKS

(Every value the average of 42 cases)

Color-Naming	Successive minutes of work									
	1	2	3	4	5	6	7	8	9	10
No. responses per min.	96	94	94	91	89	86	80	83	81	82
No. blocks per min.	3.0	2.9	3.3	3.8	4.1	3.7	4.1	3.8	4.2	4.0
Av. length blocks (in sec.)	1.9	1.6	1.6	1.7	1.8	1.8	2.1	1.9	2.0	2.0
			Av. first half = 1.7				Av. second half = 2.0			

all three studies would lead one to suspect some sort of bodily rhythm as the common cause were it not for the wide individual differences in habitual rhythms of different subjects which enter into these averages.

Individual differences. This experiment is particularly interesting in the light it throws on the correlation between an individual's speed of work and his tendency to long or short, frequent or infrequent blocks. Correlations were computed from the 21 *Ss* with the following results: correlation between speed of work and frequency of blocks equals −0.33; correlation between speed of work and length of blocks equals −0.70. Apparently these results, which agree with those of Experiments I and II, mean that the same factor which is responsible for rapid mental functioning tends also to minimize the need for the enforced rests or blocks. If there is any relation between errors and blocking, then this agrees with the well-known experimental fact that fast workers tend also to be more accurate than slow workers, and less variable in performance.

EXPERIMENT IV. SUBSTITUTION

The task for Experiment IV consisted in orally substituting letters for numbers in a series of rows of scrambled digits between 5 and 9. The letters substituted were: a for 5; b for 6; c for 7; d for 8; and e for 9. The simplicity of the code tended to cut down on the amount of practice necessary to master it, while the homogeneity offered by the limited number of items enabled the principle of refraction to operate. *E*'s assistant was supplied with a sheet containing the correct code substitutions so that errors could be recorded accurately.

Twelve new *Ss* were used. They served two days in a practice series and then 3 days in the experimental series. They worked for 10 min. every day at top speed. Since the effect of practice was great during the first two days of the experimental series only the third day's record is used in Table V. This again shows the effect of fatigue on response frequency and on frequency and length of blocks.

There is a decrease in speed from 92.6 to 83.3; an increase in frequency of blocks from 2.0 to 2.8; an increase in length of blocks from

TABLE V

SHOWING EFFECT OF FATIGUE ON RESPONSES AND ON FREQUENCY AND LENGTH OF BLOCKS

(Every value the average of 12 cases)

Substitution	Successive minutes of work									
	1	2	3	4	5	6	7	8	9	10
No. responses per min.	92.6	86.6	88.6	89.3	88.4	88.1	83.8	86.3	81.8	83.3
No. blocks per min.	2.0	2.5	.19	2.4	2.2	2.7	2.6	3.0	2.7	2.8
Av. length blocks (in sec.)	1.3	1.6	1.7	1.6	1.9	1.9	2.2	1.9	2.0	1.9

1.3 to 1.9 sec. Perhaps the only marked difference between these results and those of the preceding experiments is in the average frequency in blocking. Whereas in the earlier experiments the blocks occurred on the average every 17 sec. in this experiment they occurred every 24 sec. The difference is not very great, however, when the stage of practice is taken into account. The extent to which practice operates is shown in the day to day reduction in the length of the blocks: on day 1, the blocks averaged 2.8 sec.; on day 2, 2.1 sec., and on day 3, 1.8 sec.

The same general rhythmicality of blocking obtains in this experiment as in the first three reported. The individual records were examined and correlations computed between speed of work and frequency of blocks, as well as length of blocks. The coefficients are as follows: correlation of speed with frequency of blocks equals -0.35; correlation of speed with length of blocks equals -0.60. This is in agreement with the results already mentioned.

Bunching. In this experiment and the following one, a special series of experiments was conducted to determine whether, when the course of responses is studied over short intervals of time, any tendency toward a characteristic wave would appear. A casual observation of the records shows that between each two blocks there is a tendency for the responses to bunch up, being most frequent about halfway between blocks, and gradually thinning out as the next block is approached. It was therefore considered possible that the responses over a short period would represent the alternate crest and trough of a wave of speed or efficiency, having its low point at the block and its crest between blocks. The records would then represent a series of such waves. A question arose as to whether these apparent waves would turn out, on close study, to be formed like sine waves, with a very gradual approach to crest and trough, or whether the troughs would be rather abrupt, and the crests full and rounded.

In order to bring out all these characteristics distinctly, it was necessary to run the kymographs at high speed, so that the responses would be separated at a sufficient distance to be accurately measurable with a millimeter rule. For this purpose, every one of the 12 *Ss* furnished 4 additional 1-min. records, one on an unpracticed function, another on the same function after practice; one at the beginning and one at the end of a 10-min. work-period. It is on the basis of these short records that the following statements are made, although they can be corroborated by applying a magnifying glass to any of the preceding records.

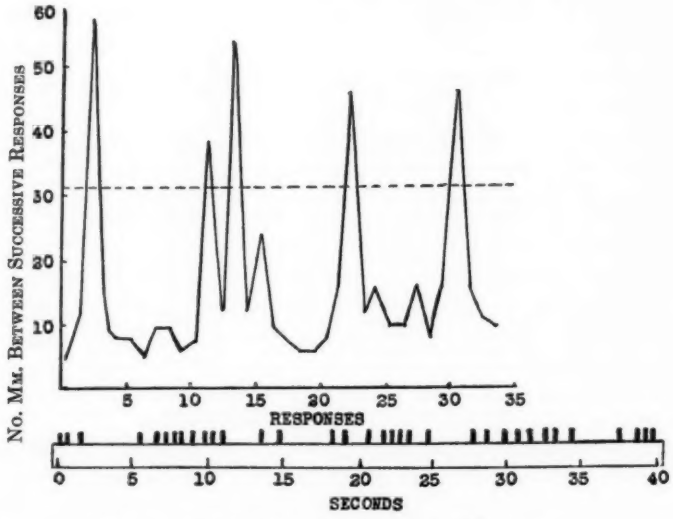

Fig. 2. Appearance of Blocks Before Practice

The following characteristics of the bunching phenomenon were examined: (1) the degree of regularity in the alternate recurrence of bunches and blocks, (2) the actual shape of a single wave; and (3) the influence of practice and fatigue on the shape and regularity of the waves.

In Figs. 2 and 3, the results are represented in two ways. The graphs show the separate responses plotted against their distances apart, represented by height on the ordinate axis. Every peak rising above the horizontal dotted line fulfills the criterion of a block. The peaks, then, represent the blocks, and the valleys between represent the bunches of responses occurring close together. It can be seen at once that the approaches to the blocks are abrupt, and that the resulting waves are not sinusoidal in shape, but can be best described as scallops. Fig. 3 shows a record after practice, while Fig. 2 shows the same person's performance before practice. Fatigue and practice have no great effect on the shape of the

waves, but practice tends to make the peaks occur much less often, and fatigue has the effect of increasing their frequency somewhat, and of accentuating their crests. See also Figs. 4 and 5.

The other part of Figs. 2 and 3 is merely an enlarged reproduction of the actual records from which the graphs were computed. The general effect is that

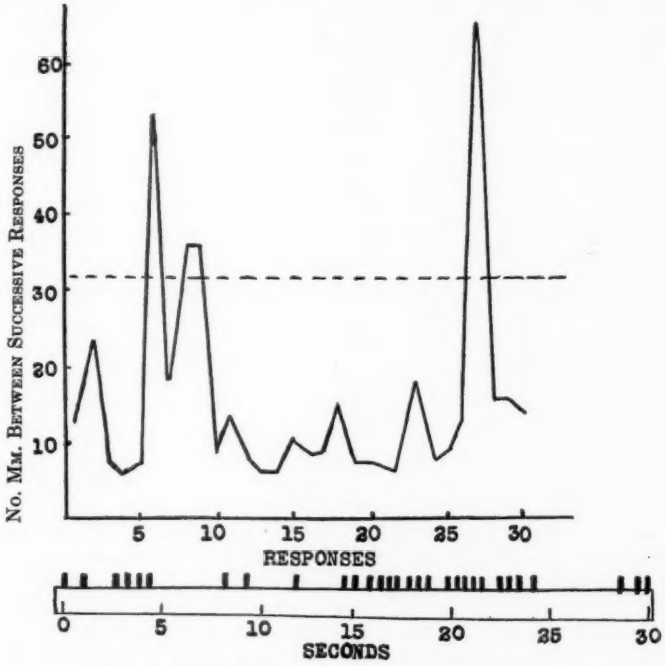

Fig. 3. Appearance of Blocks After Practice

of an alternate condensation and rarefaction, like that of air waves, except that the rarefactions terminate in actual gaps. These records are not unusual, but are quite typical of the general character of the data. The differences among the *Ss* lie in the frequency and height of the crests rather than in the novelty of the general contour.

Experiment V. Opposites

The material consisted of a sheet containing 100 words, to which opposites can be given, taken from the lists published by Whipple,[2]

[2]G. M. Whipple, *Manual of Mental and Physical Tests*, 1910, 323-326.

and arranged in such a way that the difficulty is equal throughout. *S* was furnished with such a sheet, and told to give the opposites orally as fast as he could. When he finished the sheet, he was instructed to start at the beginning again. An assistant was given a sheet containing the list of correct opposites, so that he could check every error immediately by pressing an electric key which controlled a recording needle. *E* registered the responses as usual.

The *S*s (12 in number) used in this experiment were the same that served in Experiment IV. They reported on 3 days and worked for 10 min. on each day. The results were given the same analysis as those of Experiment IV. Hence detailed tables are omitted.

In general, the findings are identical with those of the previous experiment. A practice period of 3 days increased the speed from an average of 43.3 to 62.5 responses per min. It reduced the length of the blocks from an average length of 3.9 to an average of 2.8 sec. The frequency of the blocks was reduced from 2.3 per min. on the first day to 1.8 on the second day, but on the third day it rose to 2.5. An examination of the average work curve for the 10-min. period reveals almost no decrement in speed. It is probable that the decrement was concealed by the large amount of practice still occurring during the third day.

Individual differences. When the individual results for speed were correlated with the results for frequency and length of blocks, the following coefficients were obtained. Correlation between speed and number of blocks: 0.50; correlation between speed and length of individual blocks: −0.88. This is in entire agreement with the results of Experiments III and IV, and, taken in conjunction with them, leaves little doubt of the fact that the faster worker displays fewer and shorter blocks than the slow worker.

Errors. In this study, as in the preceding, attention was given to the careful recording of errors. The object was to determine to what extent errors fall immediately before or immediately after blocks; the assumption being that such a position would indicate that the errors are a result of the low degree of efficiency at which the organism is functioning when the block occurs. Obviously the errors cannot occur during the blocks because blocks represent periods of no response. Now if chance were the only factor operating, we would expect, considering the length of time occupied by the blocks as compared with the time between successive blocks, that there would be no more than one-fifth of the errors opposite the beginning or end of the blocks. This is inferred from the fact that blocks occupy less than one-fifth of the total space. Hence, if more than one-fifth of the errors occur in conjunction with blocks it is evidence of more than a chance relation between them.

As a matter of fact, the errors are so extremely infrequent on many of the records that little can be inferred from them. But on those records which show

any appreciable number of errors, the tendency is for about half the errors to occur immediately before, or immediately after the blocks. Thus in one record there are 19 errors in all, and 9 of them fall close to blocks. In another record of 14 errors, 8 are placed near the blocks.

Experiment VI. Continuous Work for an Hour

The infrequency of the errors in the preceding experiments led to the performance of another experiment, in which the *S*s worked steadily for 1 hr. We expected that accumulating fatigue would increase the number of errors, and that more reliable statistics regarding the errors would be obtained.

TABLE VI

Showing Number of Blocks in Every Minute in an Hour's Record

(Continuous Work: Substitution or Color-Naming)

Min.	No. blocks	Min.	No. blocks	Min.	No. blocks	Min.	No. blocks	Min.	No. blocks
1	2	13	5	25	5	37	7	49	6
2	3	14	3	26	5	38	5	50	4
3	3	15	5	27	6	39	4	51	6
4	3	16	3	28	7	40	3	52	4
5	2	17	4	29	8	41	9	53	4
6	4	18	5	30	7	42	7	54	6
7	6	19	5	31	5	43	6	55	5
8	5	20	5	32	5	44	8	56	8
9	5	21	5	33	5	45	6	57	9
10	3	22	4	34	7	46	8	58	12
11	3	23	6	35	8	47	6	59	10
12	4	24	6	36	3	48	4	60	7

The continuous work records were obtained from 12 *S*s. Half of the *S*s worked on substitution and half on color-naming. They were instructed to work at maximum speed. Special care was taken to record the errors for 6 of the *S*s.

Figs. 4 and 5 give a comparative picture of the first and last sections of the record of one *S* (*B*), and bring out clearly some of the changes that occur as a result of fatigue. The size of this *S*'s blocks increased from an average of 2.1 sec. during the first half hour to to 3.2 sec. during the second half hour.

Table VI gives a detailed report of *B*'s record, showing number of blocks in successive minutes. During the first 15-min. period *B* averaged 3.6 blocks per min.; during the second, 4.9; during the third, 5.9; and during the fourth, 6.4. The size of his blocks increased during that time from an average of 1.9 to 2.4 sec., which is not great, but his speed remained practically unchanged throughout, showing

that the increase in frequency and size of blocks completely compensated for the increasing fatigue. The nature of the change is shown in the greater bunching, and less regularity of response. The responses toward the end of the hour came in short spurts, or throbs of energy, whereas at first they flowed along in a more regular stream. For example, during the first few minutes there was never a concentration of more than 16 responses in a 10-sec. period, but toward the end of the hour it was not at all uncommon to find 23 responses

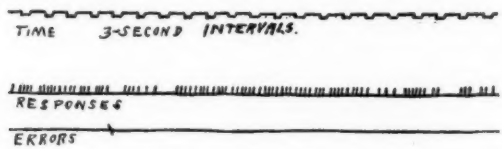

FIG. 4. SECTION FROM HOUR RECORD
(From first 15-min. period. Subject B)

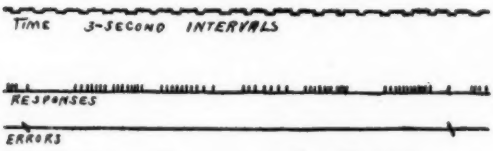

FIG. 5. SECTION FROM HOUR RECORD
(From last 15-min. period. Subject B)

crowded into a 10-sec. period. Why, then, was the speed the same throughout? Because, between these spurts, in the latter half of the hour, there were frequent blocks and thin spots. Thus, whereas in the first half there may be a stretch of 40 responses flowing along quite regularly for half a minute, in the latter half there is seldom a stretch of over 23 consecutive responses without a break, and the rule is for these to be crowded into 10-15 sec.

A comparison of Figs. 4 and 5 shows clearly the tendency, developing with fatigue, to work in short gasps, with blocks between. In Fig. 4, there is a single stretch of 50 responses without a real block. In Fig. 5, the longest stretch is of 15 responses.

Errors. An examination of the hour records for errors corroborates the findings of the previous experiment. In Table VII are listed the subjects for whom error records were kept, with the proportion of their errors falling opposite the beginning or end of a block.

Evidently not all errors occur in conjunction with blocks. This suggests the hypothesis, borne out by the *Ss'* volunteer introspections, that there are at least two different kinds of errors: (1) errors caused by confusion, of which the *S* is well aware, and which may be responsible in part for the blocks with which they occur, expecially when they stand at the beginning of the block; and (2) errors resulting from too rapid responding, of which the *S* never becomes aware, because he is going too fast. The latter kind may appear in the middle of a bunch. Occasionally an error would cause an *S* to become confused and to hesitate before going on, thus introducing an emotional block. Usually these emotional blocks

TABLE VII

SHOWING RELATION OF ERRORS TO BLOCKS

(Continuous Work)

Subject	Total Errors	Errors at blocks	
		No.	Percent
D	123	77	62.6
R	89	47	52.8
T	70	56	80.0
W	20	14	70.0
P	15	10	66.6
C	7	5	71.4
			Av. = 67.2

were of longer duration than the average. This explanation could, however, account for only a few of the blocks, since the actual number of blocks exceeded the number of errors from four to twenty times.

CONCLUSIONS AND INTERPRETATION OF RESULTS

A summary of the results of this study leads to the following conclusions.

(1) In mental work involving considerable homogeneity and continuity, there occur, with almost rhythmic regularity, blocks or pauses during which no response occurs. These blocks occupy the time of from 2-6 responses. They have an average frequency of about 3 per min., although individuals differ in this respect.

(2) Practice tends to reduce the frequency and size of the blocks.

(3) Fatigue tends to increase the frequency and size of the blocks, producing a greater irregularity in the flow of responses without reducing the actual number of responses per min. to any extent, over periods up to one hour.

(4) The responses between the blocks tend to bunch toward the center, so that a regular wave-like effect of rarefaction and condensation, alternating, is produced; the wave is scalloped in formation, rather than sinusoidal. Fatigue tends to exaggerate the bunching.

(5) Individuals who respond rapidly tend to have less and shorter blocks than slow individuals.

(6) There is a consistent tendency for errors to occur in conjunction with blocks, suggesting that the cause of errors lies in the recurrent low condition of neural functioning which the blocks reveal.

In interpreting the results of this investigation, certain things stand out rather prominently. First, it is almost certain that we have here the cause of the failure to find any great decrements in mental work as usually studied. The rests afforded by these blocks keep the individual's objective efficiency up to an average level in spite of the changes which fatigue has wrought in his nervous system. The fact is, that with this automatic safeguard operating it is practically impossible to make a subject respond absolutely continuously, and hence to produce objective decrement. Secondly, as to the interpretation of the neural mechanism involved, it appears to be related to the neurological phenomenon of refractory phase. However, since the refractory period does not follow every response but rather occurs after every group of twenty or so responses, it seems necessary to associate it with cumulative rather than simple refraction. The neurological findings of Lashley and others, tending to discredit the notion of operation of isolated neural arcs in responses of the central nervous system, suggest that we have here, not a refraction developing in a simple arc, but rather a periodic breakdown in the central facilitative mechanism, commonly called an attention wave. This periodic refraction would account for the blocks and also for the errors accompanying them.

It would be remiss to fail to mention in this connection the hypothesis of Spearman regarding oscillation, a principle which he supposes to characterize each individual's responses, *i.e.* more or less rhythmic fluctuations in the available mental energy supply or 'power' of the individual.[3] The experimental basis for the hypothesis in so far as it is based on the studies of his pupil, J. C. Flügel,[4] is, however, rather questionable, since the fluctuations reported in that series of studies are obviously artifacts produced by the method of marking time and not true 'power' fluctuations.

The findings of Kraepelin and his students should also be mentioned;[5] however it is evident that they are dealing with a quite different phenomenon from

[3]C. Spearman, G and after: a school to end schools, in *Psychologies of 1930*, 358-359.

[4]J. C. Flügel, Practice, fatigue, and oscillation, *Brit. J. Psychol. Monog.*, 4, 1928, (no. 13), 1-20.

[5]G. von Voss, Über die Schwankungen der geistigen Arbeitsleistung, *Psychol. Arb.*, 2, 1899, 399-449.

the one reported here, since their techniques could not very well bring out the latter rhythms.

Finally, the relation of the present phenomenon to the historical attention wave is doubtful. In the first place, the time relations are quite different. In the second place, the traditional attention wave turned out to be a fluctuation in the clearness of minimal stimuli and differed in terms of the sense modality investigated and the stimulus, and had a very doubtful relation to any central factor.[6] In fact, it seemed to be conditioned by factors peculiar to the peripheral nature of the sense organ. As to the relation of the blocking phenomenon to physiological rhythms, the author attempted in a tentative way to correlate the waves with such well known rhythms as the Traube-Hering wave, breathing rate, etc., by running simultaneous records of these physiological changes along with the response records. No relation could be found, though there is room for more careful work in this direction.

[6]M. L. Billings, The duration of attention, *Psychol. Rev.*, 21, 1914, 121-125; C. E. Ferree, The fluctuations of liminal visual stimuli of point area, this JOURNAL, 24, 1913, 378-409; J. P. Guilford, 'Fluctuations of attention' with weak visual stimuli, *ibid.*, 38, 1927, 534-583.

THE ACHROMATIC SENSITIVITY OF THE DARK-ADAPTED RETINA IN RELATION TO STIMULUS-DISTANCE

By Ellis Freeman, University of Louisville

In their work on chromatic sensitivity,[1] Ferree and Rand give values which are likely to be regarded as physical constants of the retina. All their observations were made at a stimulus-distance of 25 cm. The implication of their work, unconsciously based on the assumptions of geometrical optics, is that any other distance would give the same results provided the size of the stimulus were varied correspondingly. They have made determinations of the liminal intensities for spectral stimuli for the retina from fovea to extreme periphery. Since they have not dealt with the influence of stimulus-distance, it is to be taken for granted that they have assumed that this distance is indifferent provided the size of the stimulus is proportionately varied. They have, therefore, assumed that changes in external conditions will not influence the results so long as a constant image is delivered to the same retinal spot. That this classical assumption is untenable *in relation to visual acuity*, or the form sense, appears from work recently published.[2]

The problem at present is whether the assumption is tenable in relation to sensitivity, or the light sense. The anomalies of visual acuity arising from changes in stimulus-distance, despite compensatory alterations in the size of the stimulus to preserve constancy of retinal stimulation, have suggested the desirability of determining whether the chromatic and achromatic sensitivity of the light- and dark-adapted retina is not also a function of the stimulus-distance as acuity was found to be. In a paper on a preliminary investigation of chromatic sensitivity it was shown that, with reflected light, small near stimuli elicited greater sensitivity (required a smaller retinal image) than large distant stimuli of the same quality and illumination.[3] This work was done outdoors with colored papers, and served only for a rough orientation of the facts of the problem. The present study is a beginning toward making determinations, under accurately controlled laboratory conditions, of chromatic and achromatic retinal sensitivity for both light- and dark-adaptation. Because of the smaller number of technical difficulties in dealing with achromatic light, this beginning consists here of a determination of the sensitivity of the dark-adapted retina, to achromatic light, as a function of stimulus-distance.

Hitherto sensitivity has been measured by the employment of a stimulus of constant diameter but of variable intensity (and always, of course, at one fixed

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[1]C. E. Ferree and Gertrude Rand, Chromatic thresholds of sensation from center to periphery of the retina and their bearing on color theory, *Psychol. Rev.*, 26, 1919, 16-41.

[2]Ellis Freeman, What does a test of visual acuity measure? *Arch. Ophth.*, 2, 1929, 48-56; Anomalies of peripheral visual acuity, *J. Exper. Psychol.*, 12, 1929, 324-340; Anomalies of visual acuity in relation to intensity of illumination, this Journal, 42, 1930, 287-294.

[3]Ellis Freeman, An anomaly of foveal color perception, this Journal, 41, 1929, 643-645.

distance from the eye). Thus the intensity required for stimulation became a measure of sensitivity. But still another method, as far as the literature shows hitherto unused, is possible: intensity may be kept constant and the diameter of the stimulus varied. Here the diameter required for stimulation could be regarded as a measure of sensitivity. That the results from the two methods would be equivalent may not be assumed *a priori* on purely logical grounds. Such results may be a function of method,[4] and little can be said concerning them without experimentation. In the work reported here, sensitivity was measured by variations in the diameter of a stimulus of constant intensity. Furthermore, the necessary diameter was determined for stimulus-distances of 1, 2, 3, 4, 5, and

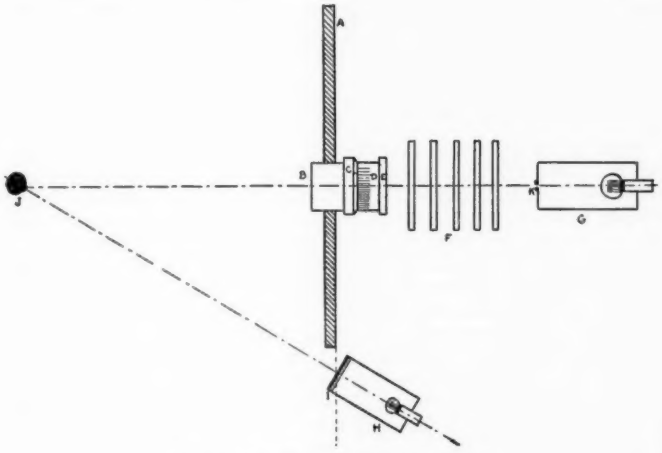

FIG. 1. DIAGRAM OF THE APPARATUS

6 m., and at 0°, 5°, 10,° 15°, and 20° of the temporal meridian of the retina of the right eye. Although a large dark-room was used, the dimensions involved were so great that no space remained for dealing with retinal spots at more than 20° of eccentricity.

Apparatus. The apparatus, diagrammatically depicted in Fig. 1, was especially constructed. A is a large black wooden screen that shuts off all stray light from the observer. In its center, and at the eye-level of a seated person, is set B, an excellent photographic iris diaphragm, constructed of so many leaves that whether open at maximum (40.22 mm.) or minimum (3.81 mm.) the aperture retains a perfectly circular appearance. C is a revolving collar which varies the diameter of the aperture. It is worked by hand with perfect freedom from backlash. D is the drum of the diaphragm bearing 24 graduations, against each of which the diameter of the corresponding aperture was determined in thousandths

[4]See 2nd article in footnote 2.

of a mm. by means of a Gaertner microscope micrometer. Interpolations were made for the middle-position between each pair of graduations. Thus a table of 47 values was established representing, to the thousandth of a mm., the diameters of the aperture from minimum to maximum extent. E is a Wratten daylight filter, No. 78AA, which, with a tungsten Mazda lamp of the kind employed, gives achromatic light. F is a battery of 5 sheets of fine ground-glass for reducing intensity and for diffusion in order to render the stimulus surface homogeneously luminous. G is the source of illumination, a light-tight tin box containing a 10 w. Mazda daylight lamp, operated at 40 volts D.C. K is a slit 16 mm. long and variable in width through a range of 50 stages from 0 mm. to 4.330 mm. In the present experiment it was kept constant at the first stage (0.087 mm.) For the work at 0° (fovea) this part of the apparatus alone was employed. For the eccentric observations, the addition was made of the light-tight tin box H, which supplied a red light-fixation-point. The redness of the light-fixation-point was secured by placing a piece of red glass over the little hole. Obviously the redness of the fixation-point made it easier to insure rigid foveal fixation on the point. For different angles, H was moved into positions along the dotted line.

Since the sensitivity determinations in the present work are represented in terms of diameter of stimulus, the intensity of the stimulating surface has no direct bearing on our data. It suffices to know that it was constant, and so very low as in no way to disturb the retinal dark-adaptation. For the sake, however, of reproducibility of conditions and also for completeness of record, the determination of intensity of the stimulating surface exposed by the aperture of the diaphragm was made. Elaborate precautions were taken to insure the highest degree of accuracy; but as the ultimate intensity to be measured was so extremely minute, the method necessarily had to be indirect, involving step-down stages and the establishment of coefficients of transmission through screens. In such a procedure the percentage of error gets multiplied from step to step, and therefore it is probable that the percentage of error in the final result, 0.000000402 candle-power total illumination, is high. This value, divided by 12.705 sq. cm. (photometrized stimulus area) gives 0.000000032 candle power per sq. cm. of stimulus. An attempted determination by means of a photo-electric cell proved futile, because the stimulus, though luminous enough to give sensation, was too weak to activate the photo-electric cell.

Procedure. Observations were made in the absolute dark, *S* being first dark-adapted for 30 min. He was seated, with his head in a heavy portable head-rest standing on the floor. The right eye was perpendicularly above a mark on the floor representing 1, 2, 3, 4, 5, and 6 m. stimulus-distance, respectively. The accuracy of this position was determined by a plumb-bob. The line of regard from the right eye to the center of the stimulus was horizontal. The left eye was kept completely covered. Before *S* was shifted to a new position, which necessitated the use of a weak flashlight, he put on a heavy blindfold mask in order to preserve his dark-adaptation.

Several weeks were first spent in training the *Ss*—5 in number and designated as *L*, *M*, *N*, *O*, and *P*. Every *S* made at least two complete sets of preliminary observations, which are *not* reported with the data of this paper. When the *Ss*, as shown by the high degree of constancy of results, were considered trained, the

TABLE I

SHOWING THE DIAMETER OF THE STIMULUS AT THE VARIOUS DISTANCES AND ANGLES OF FIXATION, AND THE CORRESPONDING SENSITIVITY OF THE *Ss*

(Stimulus-diameter given in mm.; fixation-distance in m.)

The diameter determined at 1m. represents unit sensitivity. Sensitivity was calculated for every stimulus-distance and angular fixation by the formula T/A = S, in which T is the theoretical diameter (*i.e.* the diameter at 1 m. times the distance of the stimulus in meters), and A is the diameter empirically determined.

S	Stimulus Distance	0°		5°		10°		15°		20°	
		Diam.	Sen.	Diam.	Sen.	Diam.	Sen.	Diam.	Sen.	Diam.	Sen.
	1	4.7	1.0	3.9	1.0	7.4	1.0	9.0	1.0	10.7	1.0
	2	12.6	0.8	9.0	0.9	10.7	1.4	12.6	1.4	14.2	1.5
*L**	3	20.3	0.7	12.6	0.9	15.6	1.4	19.0	1.4	20.3	1.6
	4	23.4	0.8	22.1	0.7	22.1	1.3	23.4	1.5	25.1	1.7
	1	3.9	1.0	12.6	1.0	17.2	1.0	19.0	1.0	20.3	1.0
	2	12.0	0.6	17.2	1.5	19.6	1.7	22.1	1.7	24.3	1.7
M	3	21.0	0.6	19.0	2.0	21.1	2.3	25.1	2.3	28.6	2.1
	4	27.0	0.6	24.4	2.2	26.0	2.6	29.2	2.6	29.9	2.7
	5	32.4	0.6	26.0	2.4	28.6	3.0	30.8	3.0	33.9	3.0
	6	37.5	0.6	29.2	2.6	30.8	3.4	33.0	3.4	38.6	3.2
	1	5.4	1.0	9.9	1.0	10.7	1.0	13.4	1.0	15.6	1.0
*N**	2	17.2	0.6	17.2	1.1	18.0	1.2	19.6	1.4	22.1	1.4
	3	25.1	0.6	26.0	1.1	26.9	1.2	28.6	1.4	29.2	1.6
	4	28.6	0.7	29.2	1.4	29.9	1.4	31.6	1.7	33.0	1.9
	1	3.8	1.0	7.4	1.0	11.7	1.0	15.6	1.0	18.0	1.0
	2	9.0	0.8	12.6	1.2	15.6	1.5	17.2	1.8	19.6	1.8
O	3	14.2	0.8	20.3	1.1	22.1	1.6	23.4	2.0	25.1	2.2
	4	17.2	0.9	23.4	1.3	24.3	1.9	28.6	2.2	29.9	2.4
	5	23.4	0.8	27.0	1.4	28.6	2.0	29.9	2.6	31.6	2.8
	6	27.0	0.8	28.6	1.5	29.9	2.3	33.0	2.8	38.6	2.8
	1	4.0	1.0	10.7	1.0	12.6	1.0	21.2	1.0	27.0	1.0
	2	18.0	0.4	14.9	1.4	22.1	1.1	25.1	1.7	29.9	1.8
P	3	28.6	0.4	19.0	1.7	25.1	1.5	27.0	2.4	30.8	2.6
	4	33.0	0.5	23.4	1.8	27.8	1.8	29.9	2.8	33.0	3.3
	5	36.4	0.6	27.0	2.0	29.9	2.1	33.0	3.2	35.6	3.3
	6	40.2	0.6	31.6	2.0	33.0	2.3	36.4	3.5	39.4	4.1

**L* and *N* reached the limit of response, under the conditions of the present experiment, when the stimulus was at a distance of 4 m.

final observations for use as data were made. For each distance at each angle 10 observations were made. Of these the first 5 were regarded as practice and ignored, the second 5 as data and so recorded. In Table I each item under the caption 'Diam.' represents the average of each of these second sets of 5 observations. For the observations of this second set, the stimulus was enlarged to somewhat more than was found necessary in the practice set, and then gradually reduced in diameter until reported lost. The *Ss* had practiced and acquired the knack of retaining constant fixation, and were instructed to report occasional unavoidable deviations.

Results. (*1*) *Sensitivity as a function of stimulus-distance.* Table I and the corresponding curves in Fig. 2 show first the general circumstance that retinal

sensitivity is a function of stimulus-distance. More specifically, for 0° (fovea) it appears that sensitivity *decreases* as stimulus-distance *increases*. This harmonizes with the results of the rough outdoor experiment with colored papers.[5] If at 0° (fovea) the sensitivity at 1 m. of stimulus-distance be taken as unity, then at 6 m. it drops to a level of from 0.6 to 0.8, the average for the 3 *S*s who were able to reach this distance, being 0.7. It is of further interest that the greatest drop in sensitivity occurs at 2 m., the general tendency thereafter being to remain on the same level or to rise somewhat.

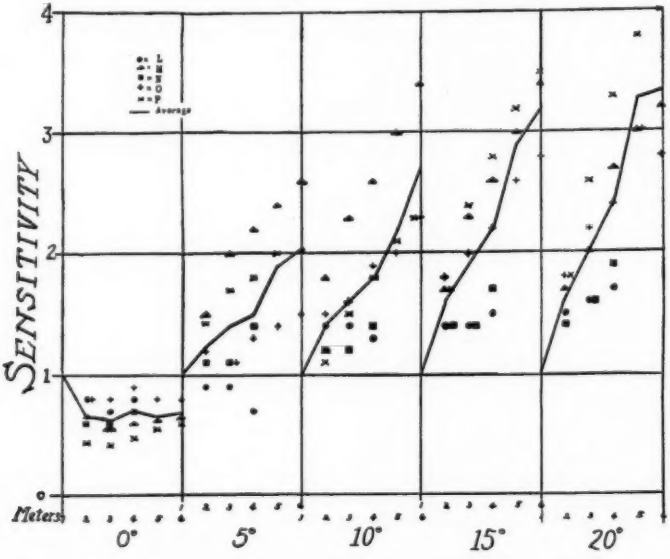

FIG. 2. GRAPHIC REPRESENTATION OF THE DATA OF TABLE I.

For the eccentric positions (5°-20°), too, sensitivity appears as a function of stimulus-distance, but in a reverse sense to the results in the fovea. For in the eccentric spots sensitivity *increases* with the *increase* in stimulus-distance. The data in Table I and the corresponding curves of Fig. 2 bring this out very clearly. The most striking instance is that of *P* in his observations at 20° and 6 m., where sensitivity had increased four-fold. For *L* the sensitivity at 5° tended to decrease with the increase of stimulus-distance, and the eccentric increase with stimulus-distance did not begin until 10° was reached. This involves no difficulty, since it may be explained as evidence of a more extensive foveal region.

[5]See footnote 3.

TABLE II

SHOWING THE SENSITIVITY OF THE TEMPORAL MERIDIAN OF THE RIGHT RETINA

The values given here are derived from the data in Table I, by application of the formula $A_0/A_x = S$, in which A_0 is the diameter of the stimulus at the fovea (0°) and A_x is the diameter at the various angular fixations. In every case the diameter of the stimulus at the fovea is taken as unity.

S	Stimulus Distance	0°	5°	10°	15°	20°
L	1	1	1.2	0.6	0.5	0.4
	2	1	1.4	1.2	1.0	0.9
	3	1	1.6	1.3	1.0	1.0
	4	1	1.0	1.0	1.0	1.1
M	1	1	0.3	0.2	0.2	0.2
	2	1	0.7	0.6	0.5	0.5
	3	1	1.1	1.0	0.8	0.7
	4	1	1.1	1.0	0.9	0.9
	5	1	1.2	1.1	1.0	1.0
	6	1	1.3	1.2	1.1	1.0
N	1	1	0.5	0.5	0.4	0.3
	2	1	1.0	1.0	0.9	0.8
	3	1	1.0	0.9	0.9	0.9
	4	1	1.0	1.0	0.9	0.8
O	1	1	0.5	0.3	0.2	0.2
	2	1	0.7	0.6	0.5	0.5
	3	1	0.7	0.6	0.6	0.6
	4	1	0.7	0.7	0.6	0.6
	5	1	0.9	0.8	0.8	0.7
	6	1	0.9	0.9	0.8	0.7
P	1	1	0.4	0.3	0.2	0.1
	2	1	1.2	0.8	0.7	0.6
	3	1	1.5	1.1	1.1	0.9
	4	1	1.4	1.2	1.1	1.0
	5	1	1.3	1.2	1.1	1.0
	6	1	1.3	1.2	1.1	1.0

Another significant characteristic is the *gradualness* and constancy of the increase in eccentric sensitivity with increased stimulus-distance as compared to the *suddenness* of the decrease of foveal sensitivity at 2 m.

(*2*) *Sensitivity as a function of eccentricity.* Table II and the corresponding curves of Fig. 3 show first that sensitivity is a function also of the degree of eccentricity. If sensitivity values be taken for any one stimulus-distance in Table II, it will readily be seen that sensitivity decreases quite regularly from fovea to periphery. This fact is well known; but in addition something new appears. That is that only at a stimulus-distance of 1 m. is the decrease very rapid from 0° to 20°. At the other stimulus-distances it is very gradual indeed. Thus it appears that not only is sensitivity, as has long been known, a function of eccentricity; but also that the *rate of change of sensitivity* from one spot to another of greater eccentricity is not a constant, but a function of stimulus-distance.

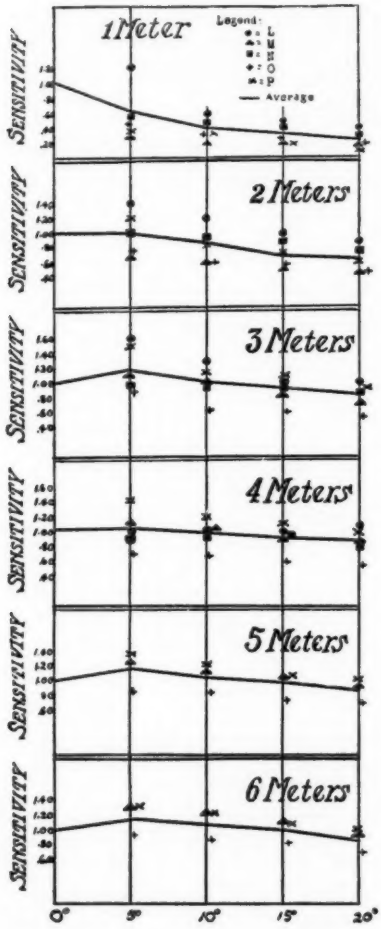

FIG. 3. GRAPHIC REPRSENTATION OF THE DATA OF TABLE II.

(3) *The contradiction between quantitative results and qualitative reports.* The presence of a contradiction between quantitative results and qualitative reports is noteworthy. Eccentric stimulation, even at 20°, was always reported as 'brighter' than foveal stimulation. This was to be expected. But what is surprising is that greater qualitative brightness did not enable the *S*s to accept a smaller stimulus than in the fovea. A slight reduction of this character did occur, however, in several places, especially for *L* and *M*, as Table II shows. Nevertheless the characteristic tendency, as the curves in Fig. 3 show, is for sensitivity to go down, especially between 5° and 20°.

Conclusion

This study has brought out two facts related to sensitivity, which may be added to those facts already brought out in relation to acuity, and which contradict the classical assumptions of geometrical optics.[6]

(1) Table I and Fig. 2 show that the sensitivity to achromatic light of a given retinal spot is not constant, but is a function of stimulus-distance: (a) foveally, sensitivity *decreases* with *increased* stimulus-distance; and (b) eccentrically, at any one given retinal spot, sensitivity *increases* with *increased* stimulus-distance.

(2) Table II and Fig. 3 show that the rate of change of sensitivity from 0° to 20° is also a function of stimulus-distance: (a) at 1 m. stimulus-distance the rate of change of sensitivity from 0° to 20° is relatively *great*, with the highest rate between 0° and 5°; and (b) at a stimulus-distance greater than 1 m. the rate of change of sensitivity from 0° to 20° is relatively *small*. Between 0° and 5° there is an increase to more than unity, thence a gradual decline.

[6]For a consideration of those influences extraneous to the retina which might conceivably account for the anomalies treated in the present paper, see E. Freeman, *op. cit.*, this Journal, 42, 1930, 293. If the influences listed there were effective, then all the anomalies could properly be regarded as artifacts, and the assumptions of geometrical optics would be preserved intact. But it may be declared at once with regard to sensitivity that dioptric and other extraretinal peculiarities of the eye are as ineffectual in accounting for the anomalies as they have been shown to be with regard to acuity.

VISUAL-MOTOR COÖRDINATION IN CONCAVE AND CONVEX MIRROR SPACE

By C. Oliver Weber, Wells College

Mirror drawing is valuable as a method of research in a surprising variety of problems. The literature regarding it, however, is scanty.[1] It has, nevertheless, won considerable appreciation as a laboratory experiment, generally eliciting the interest of students. Its usefulness in demonstrating curves of learning (with all of the attendant intricacies of plateaus), the effects of rest intervals and attitudes, etc., has been made evident by Dearborn,[2] Starch,[3] and Snoddy.[4] Although demonstration of the 'trial and error' method of learning has been its chief aim, it has been used to demonstrate the transfer of training by Starch and Dearborn and more recently by Ewert.[5] The 'psychometricians,' Burt, Yoakum, Weidensall, and others, have brought mirror drawing to bear upon the standard problems of correlation with sex, intelligence, age, and other factors.[6]

I have found occasion to employ mirror drawing in an attack upon the problem which lies at the crossroads of recent psychological research; namely, the problem of perception. Specifically, the mooted question of the bearing of kinaesthetic data upon visual space perception appealed to me as a problem upon which mirror drawing might throw some light. To this end, the utility of the mirror is at once apparent. One requires a specific experience during which the eye gets an impression which differs radically from the corresponding experience of the muscles. I shall call the space in which the hand moves 'manual space' and the space which the eye sees 'mirror space.' The space of a plane mirror varies from manual space solely in one essential regard—in the *direction* of the movements. Mirror space is conventionally Euclidean in all essential respects except as regards the direction of movements. But, not all the directions of manual space are altered by the mirror in the same way. We may distinguish two types of alteration, intra-configurational and extra-configurational. Consider first the extra-configurational alteration which a plane mirror imposes on manual space. Imagine a mirror reflecting a point of light, which moves in an otherwise empty space. The observer sees the motions of both the light and of its image. The movements occur over a formless field: What do we find? Of the three Cartesian coördi-

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[1]Carmichael, L., The history of mirror drawing as a laboratory method, *Ped. Sem.*, 34, 1927, 90.

[2]W. F. Dearborn, Experiments in learning, *J. of Educ. Psychol.*, 1, 1910, 374-378.

[3]D. A. Starch, A demonstration of the trial and error method of learning, *Psychol. Bull.*, 7, 1910, 20-23.

[4]G. S. Snoddy, Experimental analysis of a case of trial and error learning, *Psychol. Monog.*, 124, 1920, 1-78.

[5]P. H. Ewert, Bilateral transfer in mirror drawing, *Ped. Sem.*, 33, 1926, 235-249.

[6]The most complete bibliography known to the writer is that given by G. M. Whipple, *Manual of Mental and Physical Tests*, 2, 1921, 132-133.

nates, the 'up-down,' the 'right-left,' remain as they were in manual space. The point of light and its image move together so long as the motion is in a plane parallel to the mirror. Only the movements towards and away from the mirror, as Alice found in Wonderland, present an anomaly. As the light moves towards the mirror, the image in the mirror rushes forward to meet it. This is one of the sources of the subject's vexation in tracing a star. Dearborn analyses the problem as follows: "The star is printed at such an angle that no lines are exactly parallel or perpendicular to the base line of the mirror. The chief difficulty is experienced on the slanting lines."[7] In fact, in tracing any line forming part of the star the pen, though it may move also from left to right or contrariwise, also moves away from or towards the subject's body.

But the left-right movements also offer difficulties for the subject, due to the very fact, as I stated previously, that the mirror reproduce faithfully the left-right and the up-down portions of the figures reflected in it. This brings it about that clockwise order in manual space appears as counter-clockwise in mirror space. But note, that we are not now dealing with the motion of a point without a ground (extra-configurational motion), but with movements within a ground which the mirror also reflects. The dial of the clock, for instance, reflected in a mirror presents the anomaly of hands running backwards. This backward motion occurs, however, in the image of the dial, which represents, so to speak, an auxiliary space within the space of the mirror. Remove the clock, and replace once more the point of light in an empty field. Give the light a circular motion before the face of the mirror. There is no reversal of movement—the field is extra-configurational.

What complications are introduced by using spherical and concave mirrors? First of all, it must be noted that these mirrors are precisely like plane mirrors in their effects on the direction of movements. But, in addition, they introduce (a) alterations of magnitude, and (b) alterations of form. A plane figure, a square, reflected on a convex mirror surface appears reduced in size. The form is distorted due to the fact that vertical dimensions of the figure are reduced more than are the horizontal ones. A square, in such a mirror, has approximately the form of a rectangle, resting on its long side. The concave mirror (with the reflecting surface bulging away from the subject) has exactly the opposite effects. The image of the square will be enlarged, especially in the vertical line, making it appear like a rectangle standing on end. A third complication is due to the fact that the vertical distortions imposed by both the convex and the concave mirror, vary with height above the base line. So that a square is not converted into a perfect rectangle, but into a trapezoid. This effect, however, is very slight, and is ignored in this experiment. The mirrors convert a square into an approximately perfect rectangle.

With these brief notes on the properties of convex and concave mirrors in mind, the reader is perhaps prepared to agree that convex and concave mirrors should enable us to give hands and eyes quite diverse experiences of what we nevertheless call the 'same' experience. Our next concern is to arrange an experiment so that hand and eye must coördinate, under conditions which give each one an equal chance of imposing its findings on the other.

[7] Dearborn, *op. cit.*, 377.

Experiments

The subjects, 9 in number, had no acquaintance with the special problem: are the retinal or the muscular criteria *dictatorial* in visual-motor organization? To them, it was just a mirror drawing experiment, to be carried out in 5 series of observations, which form 3 logical groups. Series A and B give a simple index of *S*'s ordinary visual-motor organization. In Series A, *S* reproduced, while in the usual position before the mirror, but with eyes closed, 5 squares, every one was as nearly as possible just one inch square. In Series B we aimed to secure *S*'s notion of a square inch when the selection was visual. For this purpose 30 figures (squares and rectangles) are presented, ranging in area from 0.6 to 2.0 sq. to 2.0 sq. in. Ten of these figures were square, 10 were rectangular with the long edge horizontal, and 10 were rectangular with the short edge horizontal. The 30 figures of Series B were presented to *S* in haphazard order. *S* was required to select a figure that visually appeared an inch square. As each figure was selected, it was returned to a new position in the group before the next selection was made. Five selections were required. We were then in possession of two of *S*'s customary notions of a square inch: one motor, the other visual. The two were astonishigly unlike. The kinaesthetic figure was almost 50% smaller than the visual, although the observer intended both of them to be one inch square.

We were now prepared for the crucial series, Series C, a training series in which *S* copied model rectangles in the mirrors. One *S* used the convex, the next the concave mirror, and so on. *E* placed before *S*, one at a time, a new series of 20 figures, some square and some rectangular, ranging in actual area from 0.5 to 1.7 sq. in. During this training series, *S* saw his hand and the model square to be copied only as they were reflected in the mirror. In short, the situation was such that vision and kinaesthesis had no external access to each other's data. The eye could have no true impression of the figure which the hand copied. The hand, since it copied the real figure, received no motor experience of the reflected image of it. All the *S*s had had practice in plane mirror drawing, and square and rectangular areas are easier to draw than star figures, consequently the mere task of mirror drawing was reduced to a minimum and the *S*s were able to give their attention to the exact size and form of the figures they were copying. In short, throughout the Series C, lasting some 20 min., the *S*s moved in a new experiential world. They were obliged to draw manual figures which were quite unlike their visual models. What happened? A real rectangle, standing on end, will appear much smaller and square in the convex mirror, but in order to draw another figure that 'looks like it' in this mirror, *S* must draw a genuine rectangle, smaller than the visual image. Identity, says logic, cannot embrace contradictory differences. Perhaps not, in the sphere of concepts, but, given a little practice, the *S* finds nothing strange in drawing a rectangle which appears square. Perhaps something other than logic obtains in this strange realm which psychology designates as coördination. In a later paragraph, we may try to formulate the principles which seem to govern here.

Finally, every *S* was asked to draw 5 additional inch squares (Series D, a repetition of Series A) and to select 5 more visual figures of the same class (Series E, a repetition of Series B). Measurements in this experiment consist entirely of determinations of height, breadth, and area of the figures drawn or selected.

These data were easily secured for the model figures, which were symmetrically drawn. The figures drawn by the *S*s, however, though quite regular, were not regular enough to give reliable areas when the base was multiplied by the height. The areas of the *S*s' figures were therefore secured by means of the planimeter, adjusted to give areas in square inches. The mean between the longest and the shortest height was recorded as the true height, and the true width was determined in a similar way.

Results

We may preface this section by the general statement that the brief experience of convex or of concave mirror drawing radically alters the former visual-motor organization of every *S*. The effects were almost entirely alike for all *S*s. It is well known that plane mirror drawing, over a period of only 20 min., can alter, temporarily, the visual-motor coördinations of a lifetime. Turning from the mirror to ordinary space, *S* finds himself unable to write his name, traces a circle in an unwonted direction, etc. In my experiment, although the effects of experience in spherical mirror space are marked, their bearing upon the original question is not simple. That question was, "Do kinaesthetic criteria alter the character of the retinal impression? In fact, this question itself is not simple. There are at least two ways in which motor factors may influence the visual experience. (1) Past motor experience may influence what purports to be a visual judgment, but without influencing the actual visual phenomena. For instance, a Müller-Lyer figure may be judged 'shorter' when compared with an ordinary line; but this does not actually prove that the eye muscles have altered the extent of the retinal impression or of the visual sensation arising from it. The eye-movements might affect the judgment without exerting this influence through vision. A judgment is the result of complex influences, in this case, of both kinaesthetic and retinal criteria. Assuming that kinaesthetic influences cause the Müller-Lyer illusion, it does not follow that vision has been affected in any phenomenal way. In fact, it is notorious how the observer, looking too closely at the illusory figure, soon fails to see the distortions. (2) The possibility is that motor experiences may alter the retinal impressions.

This brief statement of a vexing problem may be resumed after a presentation of the results of the present experiment. Tables I, II, and III give, respectively, the results for the concave mirror, the convex mirror, and the training series. We read the data in Table I, which serves as a model for the rest, as follows: The changes in 'area,' 'base,' and 'height' form the major divisions of the table. Under 'area' (which serves as a model for the remaining data), three columns are given. Column O (original) gives the average size of the figures which *S* selected or drew in Series A and B. Column F (final) given the equivalent averages for Series D and E. The column '% Diff.' gives the difference between the values in columns O and F in terms of percentages. This difference is recorded + or −, depending on whether the final selection or drawing is greater or less than the original one. Thus, on the average, the 9 *S*s selected for a square inch a visual figure which was actually 1.15 sq. in. After drawing in the concave mirror, they select a figure which is 1.45 sq. in., which is larger than the original selection by 26 %.

TABLE I

SHOWING RESULTS WITH CONCAVE MIRROR

	Area			Base			Height		
	O	F	% Diff.	O	F	% Diff.	O	F	% Diff.
Visual	1.15	1.45	+26.0	27.4	30.7	+12.0	27.6	30.9	+11.9
Kinaes.	0.69	1.01	+46.4	20.7	24.8	+19.8	18.6	27.2	+46.2

TABLE II

SHOWING RESULTS WITH CONVEX MIRROR

	Area			Base			Height		
	O	F	% Diff.	O	F	% Diff.	O	F	% Diff.
Visual	1.09	1.09	0	26.6	26.6	0	26.0	26.1	+ 0.4
Kinaes.	0.87	0.97	+11.5	22.9	26.2	+14.4	24.1	24.9	+ 3.3

TABLE III

SHOWING RESULTS DURING TRAINING SERIES

(S = stimulus-dimension; R = reproduction)

	Area			Base			Height		
	S	R	Diff.	S	R	Diff.	S	R	Diff.
	0.55	0.54	− 1.8	20	20.6	+3.0	18	17.7	−1.7
Concave	1.28	1.01	−20.0	30	29.1	−3.0	28	27.0	−3.6
mirror	1.36	1.23	− 9.6	31	29.4	−5.4	29	26.9	−6.5
	1.41	1.14	−19.1	29	28.0	−3.4	28	27.0	−2.9
	0.83	0.78	− 6.0	22	23.3	+5.9	24	23.8	−1.0
Convex	1.00	0.98	−22.0	24	25.4	+5.5	26	23.9	−8.1
mirror	1.39	1.23	−11.6	27	28.6	+5.9	29	28.7	−1.0
	1.38	1.40	+ 1.5	29	30.1	+3.8	31	31.0	0.0

Major interest, of course, centers on the final as compared with the initia selections and drawings of a square inch. It is at once apparent that the brief experience of mirror drawing has modified the original perception (or conception?) of a square inch. The nature of the change depends on the mirror used, whether concave or convex, in all respects save one. This one respect is, that all the final figures tend to be larger. This fact somewhat obscures the opposing effects of the mirrors. Analysis of the data, however, serves to exhibit the opposition. The concave mirror enlarges the figures reflected into it. This causes the *S* to select, as a final inch square, an average figure that is 26% greater in area, 12% longer in base, and 11.9% higher than the original selection. But, there is a *general* tendency to draw larger figures that is here *added* to the influence of the concave mirror. The convex mirror, decreasing the size of the figures, leads *S* to draw figures that are smaller than her original ones. This latter tendency, however, is *opposed* by the general tendency to draw larger figures, as the experiment proceeds. Inspection of the raw data shows unmistakably the existence of this general tendency; the tendency, irrespective of the effects of the mirrors, to draw figures that are successively larger in size. In Table IV, the fact is exhibited for area alone, and if it holds for area, it must hold for base or height or both.

TABLE IV

INCREASES OF MOTOR ERRORS NOT DUE TO MIRRORS

	Trial	Original	Final
Visual Selection	First	0.97	1.14
	Fifth	1.30	1.29
Motor Drawing	First	0.74	1.05
	Fifth	1.05	1.26

In Table IV we find, for instance that the average of the first visual selections is 0.97 sq. in., but the fifth selection has increased to 1.30 sq. in. A similar increase occurs in the motor drawings. It occurs also in the selections and drawings, *after* the mirror drawing. What is the origin of this general tendency? Were it confined to the manual drawings, an explanation might be offered in terms of previous investigations.[8] Effort tends to appear as magnitude—as greater area and distance. Since practice tends to reduce effort (required to draw the figures of the required form and size in the mirrors) the size of the figures should gradually grow larger.

Setting aside the obscuring effects caused by the tendency to select and draw larger figures, irrespective of mirrors, we may generalize the effects of the mirrors as follows. (1) After concave mirror drawing, an *S*'s phenomenal conception of standard plane areas becomes enlarged and especially exaggerated in height (Table I). (2) After convex mirror drawing, an *S*'s phenomenal conception of plane areas becomes reduced, especially in height (Table II). (3) These effects are much more pronounced in the motor than in the visual sphere (Tables I and II). (4) These alterations occur gradually throughout the training series, in accordance with the usual process of learning.

These results are clear-cut, but their explanation is not easy. Indeed, the coöperation of hand and eye, simple as they appear in the sphere of action, are chronically opaque to reflection. So much is this the case, that it should be an unspoken assumption that all ventures in this field are at present highly tentative. The explanation of visual illusions in terms of eye-movements is banal in its familiarity. Experimentation has not succeeded in proving its truth to the satisfaction of everyone. Higginson suggests that eye-movements play a causal rôle in illusions of movement.[9] This thesis is disputed by the *Gestalt* psychologists in general. Köhler reverses the usual explanation of the Müller-Lyer illusion.[10] In place of explaining the shortened figure in terms of abbreviated eye-movements, he attributes the abbreviated eye-movements to the illusion. Guilford and Dallenbach deny the rôle of eye-movements in the two types of illusory movement known as the autokinetic and the phi-phenomenon.[11] The present

[8]C. O. Weber, Properties of space and time in kinaesthetic fields of force, this JOURNAL, 38, 1927, 597-606; C. O. Weber and K. M. Dallenbach, Properties of space in kinaesthetic fields of force, *ibid.*, 41, 1929, 95-105.

[9]G. D. Higginson, The place of ocular movements in stroboscopic perception, *ibid.*, 37, 1926, 408-413.

[10]W. Kohler, *Gestalt Psychology*, 1929, 96.

[11]J. P. Guilford and K. M. Dallenbach, A study of the autokinetic sensation, this JOURNAL, 40, 1928, 96; J. P. Guilford, Eye-movements and the phi-phenomenon, *ibid.*, 41, 1929, 595-606.

experiment does not bear directly upon the perception of visual illusions, whether of the static or of the dynamic (moving) type. This study rather belongs to the group exemplified by the studies of Stratton[12] and Wooster.[13] In these studies the object is to discover, if possible, the nature of the relation which obtains between eyes and muscles. Stratton wore lenses that gave upright images upon the retinae. The field of vision appeared inverted, and the coördination of hand and eye was seriously affected. Very quickly, a new coördination appeared. By the seventh day, in walking up and down a room, there was "perfect reality" and "complete harmony" in the *S*'s experience of his surroundings. Wooster used prismatic lenses that produced an angular shift of the visual field of 21°. As in Stratton's experiment, a new coördination appeared, which lasted several years after the discontinuance of the experiment. The manner in which these new organizations were set up suggests a great deal regarding the basic relations of sense organs to motor organs. On being restored to normal vision, the *S* in Stratton's and Wooster's experiments found himself disoriented; *i.e.* he had to learn anew the original movements necessary to handling objects. The final tendency of my *S*s to draw 'inch squares' that were either too large or too small, either too wide or too high, represents the form in which disorientation appears in this experiment.

It is difficult to understand why disorientation in my experiment takes the form discovered. In Wooster's experiment, after removing prisms that displace to the left, the *S* 'pass-points" to the right: the reason is obvious. The effects of convex or concave mirror drawing, however, do not seem to fit any simple explanation. The data is far too incomplete and tentative at this stage. New experiments are now under way, in which larger mirrors and varied procedures are being used. The use of parabolic mirrors in addition to mirrors of the convex and concave form enables the experimenter to vary the mirror drawing experiment in interesting and instructive ways.

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[12]G. M. Stratton, Vision without inversion of the retinal image, *Psychol. Rev.*, 4, 1897, 341-360.

[13]Margaret Wooster, Certain factors in the development of a new spatial coördination, *Psychol. Monog.*, 32, 1923, (no. 146), 1-96.

HANDEDNESS AND ABILITY

By M. O. Wilson, University of Oklahoma, and
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The chief purpose of this study is to present certain facts concerning the relation of handedness to intelligence and achievement scores in a group of 975 6-A pupils.[1] Some questions to be answered are: (1) What is the relation of handedness to intelligence scores, achievement scores, and chronological age? (2) When the sinistrals and dextrals are equated with respect to intelligence, how do the two groups compare with respect to achievement? (3) What sex differences are observed?

Procedure. In the Oklahoma City Schools during the academic year of 1927-1928 the following tests were given to every 6-A pupil: the Otis Self-Administering Test of Mental Ability, Intermediate Examination; the Stanford Achievement Test, Reading Examination; the Compass Survey Arithmetic Test, Advanced Examination. Ratings of the general ability of the pupils by their regular teachers were also obtained. All scores and ratings were expressed as G-scores and were to be used along with the chronological ages as a basis for homogeneous grouping in the junior high schools the following year. These data, together with an indication of the preferred hand of each 6-A pupil, constitute the basis of this study.

All of the tests were administered and scored by the regular teachers of the children concerned. These teachers were conscientious and thorough in all their work and were trained in testing and scoring technique, as a part of the administrative and supervisory program of the entire school system. In the present study it is assumed that the tests are approximately as reliable as one could obtain for the purpose desired. No attempt was made to improve their validity or reliability. All computations were performed twice on the calculator, and the results are correct to four decimal places, but for convenience in this report only two decimals are used.

Results

Incidence of left-handedness. (a) *In general.* Preliminary to the principal study to be reported here, the incidence of left-handedness and changed handedness among a large number of junior high school children was determined. The data on changed handedness were obtained by asking each child who had been

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[1]The writers wish to acknowledge their indebtedness to the following: to the teachers for their coöperation; to Dr. C. L. Cushman, Director of Research in the Oklahoma City Schools for the use of the data in his office and for suggestions; and to Elma Thompson Dolan for help in the statistical computations.

changed from a left-handed writer to a right-handed writer to indicate the fact. The total number and percentage of boys and girls who were left-handed or had been changed from left-handedness are shown in Table I.

It is not the purpose of this study to discuss the causes of handedness, but it is interesting to note, when observing the results of this table as well as those of Table II, that according to the statements of the children a substantial percentage of both boys and girls (5.23% and 4.14%, respectively) had changed from left-handedness to right-handedness as a result of social pressure. In other words, the left-handed group would have been materially larger than it really was if each child had been permitted to use the left as the preferred hand. In this connection, Watson says that handedness is not an instinct.[2] It is possibly not even structurally determined. It is socially conditioned. According to his theory handedness does not result from any innate tendency to use the one hand in preference to the other. The preference is determined by some early experience and by subsequent formation of habit based on this experience. This theory is debatable, however, and it is not our purpose to defend either side of the question.

TABLE I

INCIDENCE OF LEFT-HANDEDNESS AND CHANGED HANDEDNESS AMONG 2328 JUNIOR HIGH SCHOOL PUPILS OF OKLAHOMA CITY

Name of school	No. boys	No. girls	% left-handed		% changed	
			Boys	Girls	Boys	Girls
Capitol Hill	242	188	6.91	2.65	4.95	4.25
Roosevelt	321	335	3.73	4.17	4.36	3.28
Harding	237	286	4.64	2.44	8.01	2.09
Webster	347	372	4.03	1.34	4.32	6.45
Total	1147	1181	4.62	2.62	5.23	4.14
Total sinistrals plus changed			9.85	6.76		

Regardless of the strength of social pressure, there is, according to our data, a small group of the children who are hardy enough to continue the use of the left as the preferred hand—even in the difficult task of writing. This percentage is 4.62 for boys and 2.62 for girls. These findings correspond roughly to those of other investigators. The phenomenon of left-handedness is not confined to any one race, nationality, social group, or historical period. It has been found to exist in all nations and at all times.[3]

(*b*) *In special and regular rooms.* As to the relation of handedness to ability, a comparison first was made between the percentage of left-handedness of children in several special rooms for the subnormal on the one hand, and the amount of left-handedness found in the number of regular rooms taken at random throughout the junior high school system on the other. These results are summarized in Table II. It is seen that the percentage of left-handed children in the special rooms runs almost twice as high as that of the regular rooms. Unfortunately, the data on changed handedness in this connection were not available. But in

[2] J. B. Watson, in *Psychologies of 1925*, 27-31.

[3] The archaeologists tell us that most of the ancient instruments and tools that are unearthed in various parts of the world were obviously intended for use in the right hand, but that in all large finds there are a few designed for use in the left hand. Cf. D. Wilson, Primeval dexterity, *Proc. Canadian Inst.*, 3rd Series, 3, 1886, 125; Right- and left-handed fossils, *Lit. Dig.*, 53, 1926, 74 f.

TABLE II

INCIDENCE OF LEFT-HANDEDNESS IN REGULAR AND SPECIAL ROOMS COMPARED

Type of room	No. of rooms	No. of pupils	Number of left-handed	% of left-handed
Regular	34	1297	46	3.54
Special	10	151	16	6.37
Total	44	1448	62	4.21

the light of data in Table I, it is highly probable that there was a relatively large percentage of children who had been forced to change from the left as the preferred hand in writing. These percentages suggest that one of the contributory causes of the problem child is his lack of adjustment to a right-handed environment, both in school and out.

(c) *In sectioned classes.* The next observation in connection with relation of handedness to ability was concerned with the incidence of handedness in groups which had been sectioned on the basis of ability for the purpose of instruction and supervision. These data are summarized in Table III, and show the percentages of left-handed pupils in the high and low sections and the percentages of pupils

TABLE III

INCIDENCE OF LEFT-HANDEDNESS AND CHANGED HANDEDNESS IN SUPERIOR AND INFERIOR ABILITY SECTIONS OF 2328 JUNIOR HIGH SCHOOL PUPILS OF OKLAHOMA CITY

Name of School	No. of pupils in high sections	No. of pupils in low sections	% Left-handed pupils in		% Changed pupils in	
			High sections	Low sections	High Sections	Low Sections
Capitol Hill	220	210	4.09	5.71	3.63	5.71
Roosevelt	334	322	3.89	4.03	2.09	5.69
Harding	274	249	3.64	3.21	2.55	7.22
Webster	366	353	2.73	2.54	4.09	6.79
Total	1194	1134	3.51	3.70	3.09	6.34
Total sinistrals plus changed			6.60	10.04		

whose handedness had been changed in these sections. In the majority of cases there is a larger percentage of pupils in the low sections who are left-handed than in the high sections; and there is, without exception, a higher percentage of children in the low sections who had changed from the left as the preferred hand. Here again it might appear that left-handedness is partially responsible for inferiority in school.

General comparisons. Now that these preliminary observations are out of the way, let us proceed to the consideration of the data upon which the main study is based. These data were obtained from 975 pupils in the 6-A grade and consisted, as was stated above, of intelligence scores, reading scores, arithmetic scores and teachers' ratings on general ability, all of which were expressed in raw scores and G-scores. In addition to these, the preferred hand for each child was determined. Space will not permit the presentation of all of the data for so large a group and for so many variables. Therefore, some of them have been

condensed and summarized in terms of mean values in Table IV. For all variables except the intelligence quotient and chronological age, the scores are expressed in G-scores.[4]

It is to be observed, first of all, that the number of sinistrals in this group is only 44, a number rather small for group comparison and even smaller when sex differences are to be considered, since there were only 26 males and 18 females. But it is to be remembered that these 44 sinistrals come from nearly 1000 sixth-grade children, and are probably a very fair sample of sinistrality in the Oklahoma City system, and not merely 44 selected children. When the range and

TABLE IV

RELATIVE ABILITY OF DEXTRALS AND SINISTRALS OF THE 6-A GRADE OF THE OKLAHOMA CITY SCHOOLS

	Chronological age	I.Q.	Reading G-score	Arithmetic G-score	Ar. & Re. G-scores combined	Teach. rank G-score	Number of cases
Mean for dextrals	12.45	105.45	7.41	7.11	7.23	7.56	931
S.D. of mean	0.03	0.51	0.05	0.03	0.03	0.03	
Mean for sinistrals	12.72	102.13	7.21	6.88	7.01	7.36	44
S.D. of mean	0.15	2.48	0.26	0.17	0.19	0.21	
Difference in means	−0.27	3.32	0.20	0.23	0.22	0.20	
S.D. of differences	0.15	2.53	0.26	0.17	0.19	0.21	

class-interval frequencies of the two groups for the different variables are compared it is found that the two distributions are very similar. The principal difference lies in the facts that the central tendencies of measurement results are lower for the sinistrals, and that the sinistrals are older. Hence, in the subsequent statistical procedure, although we shall use the traditional methods of testing reliability of group differences, it should be kept in mind that the reliability of the differences is probably greater than the ratios indicate because of the generality of the sampling of sinistrals.

An analysis of Table IV reveals, first of all, that sinistrals are a fourth of a year older than dextrals. This group of sinistrals is slightly retarded at the time at which they finish the sixth grade, since our study deals only with those children who were to be promoted to the seventh grade. They are slightly lower in intelligence, namely, 3.32 points. This may be due to the fact that the test used was constructed for the convenience of right-handed rather than left-handed children. Likewise, the sinistrals are inferior in reading achievement and arithmetic achievement. Finally, it is observed that the teachers of these sinistrals rate them lower in general ability than they did the dextrals. These ratings may be somewhat attenuated as a result of the fact that the teachers are familiar

[4]G-scores or grade-scores are expressed as mixed decimals. The whole number denotes a school year or grade, while the decimal denotes a given month of that grade. For example, a G-score of 7.3 in a given test means that the performance of the individual so graded is equal to the performance of the average pupil after three months of work in the seventh grade.

with the test results of all of their pupils. It may be concluded from Table IV that the sinistrals are consistently inferior to the dextrals at the time the children concerned are promoted to the seventh grade. The differences, however, are not statistically reliable, but their consistency tends to enhance their reliability.

When the sinistrals and dextrals are equated in intelligence and then compared as to ability, it is found that there is still a constant superiority in favor of the dextrals, although the differences are slightly reduced in some instances. In equating the two groups the simple method of matching was used rather than the method of partial correlation, since the smallness of the sinistral group did not justify the labor involved in the latter method. Every I.Q. of the sinistral was matched with one or more in the right-handed group which did not differ from it by more than one point in either direction. All of the dextrals excepting 93 were so matched. A majority of these were of superior intelligence. These

TABLE V

RELATIVE ACHIEVEMENT OF DEXTRALS AND SINISTRALS WHEN EQUATED IN INTELLIGENCE

	Chronological age	I. Q.	Reading G-score	Arithmetic G-score	Ar. & Re. G-score	Teach. rank G-score	No. cases
Mean for dextrals	12.41	102.17	7.35	7.06	7.24	7.51	838
S. D. of mean	0.04	0.53	0.06	0.04	0.04	0.04	
Mean for sinistrals	12.72	102.13	7.21	6.88	7.01	7.31	44
S. D. of mean	0.15	2.48	0.26	0.17	0.19	0.21	
Difference in means	0.31	0.04	0.14	0.18	0.23	0.20	
S. D. of differences	0.15	2.53	0.26	0.17	0.19	0.21	

results are shown in Table V. It would seem from an analysis of this table that when a dextral and a sinistral are equated in intelligence the dextral will have a slight advantage in school achievement.

Correlations. In addition to comparisons of mean achievement one may determine the relation of handedness to ability by determining the degree of correlation between them. A difficulty in this connection, however, is immediately obvious; the handedness variable cannot be expressed as representing a continuous scale. It is believed, nevertheless, that it is continuous, for the following reasons. As Downey points out,[5] we ordinarily consider, in connection with handedness, only the uni-manual acts, such as writing, eating, throwing, and the like, whereas the bi-manual acts of batting, sweeping, hoeing, chopping, etc., are equally important and significant. Thus the fact that an individual throws a ball with the right hand is not sufficient reason for predicting that he will also use an axe or bat in the right-handed manner. If this is true, the formula for handedness cannot be represented merely by R (right) or L (left). Since the bi-manual acts of many individuals are divided, that is, some persons may sweep with the right hand and bat with the left hand, we must employ at least three characters to designate handedness types. We might let RRR indicate those

[5] J. Downey, Types of dextrality and their implications, this JOURNAL, 38, 1927, 317-367.

individuals who are dextrals consistently as regards both uni-manual and bi-manual acts; RLL, those who are dextral in uni-manual acts and sinistral in bi-manual responses; and RRL, those persons who are dextral in uni-manual acts but have a divided bi-manual preference. We might, likewise, let LLL, LRR, and LLR represent the corresponding types of handedness in sinistrals. Thus we may have a scale of handedness in which degrees of dextrality, let us say, can be expressed. Starting with the consistent dextral and ranging through to the consistent sinistral, such a scale would be RRR-RRL-RLL-LRR-LLR-LLL; and we might insert in the middle of this scale some symbol for the ambidextrous individual, if such individuals exist. There is thus some evidence, according to the findings of Downey and others, to justify the assumption that handedness constitutes a continuous scale.

TABLE VI

COEFFICIENTS OF CORRELATION OF HANDEDNESS WITH INTELLIGENCE, ACHIEVEMENT, AND CHRONOLOGICAL AGE (BY THE BI-SERIAL R METHOD)

	Handedness correlated with	
Intelligence, 0.09	Achievement, 0.08	Chronological Age, −0.11

But since in the present study handedness was determined by a uni-manual act, namely, writing, it is impossible to array the handedness data other than according to two categories, right-handedness and left-handedness. Hence it is necessary to use a special correlation technique in order to compute the desired coefficients; namely, the bi-serial r method.[6] The coefficients obtained between handedness on the one hand and intelligence, achievement and chronological age on the other by use of this method are given in Table VI. These coefficients are all very low; they might have been higher if handedness could have been measured in terms of a continuous scale. The consistency of the three coefficients indicates,

TABLE VII

COEFFICIENTS OF CORRELATION OF CHRONOLOGICAL AGE WITH AVERAGE ACHIEVEMENT FOR DEXTRALS AND SINISTRALS

	Average achievement correlated with chronological age	
Dextrals	−0.42	0.01
Sinistrals	−0.57	0.06

however, that there may be some relationship between handedness and ability. That is, the positive coefficients of handedness with intelligence and achievement indicate that the right-handed children make higher intelligence and achievement scores and the negative coefficient for chronological age shows that they finish the sixth grade somewhat earlier than do the left-handed children.

In connection with the latter point, namely, the relation of age to achievement in the two groups, the coefficients of correlation between these two variables are presented in Table VII. They are negative, as would be expected, but higher than is usually found for this relationship. The point of interest lies in the fact that there seems to be a somewhat closer negative relationship between those

[6]See T. L. Kelley, *Statistical Method*, 1923, 246-249.

two variables for the sinistrals than for the dextrals. That is, there is a closer association between age and retardation in achievement for the former than for the latter group.

Sex differences. As to sex differences within the left-handed group, especially, little can be gained from such comparisons, since there were only 44 sinistrals. These results will, however, be presented for whatever they are worth. It is hoped that in the near future data for additional left-handed children may be added to those of the present study, so that more reliable results may be obtained from comparisons. Since the data are too complex to present in tabular form, a few running statements concerning mean achievement of the sex groups will have to suffice. The sinistral girls are younger than the sinistral boys by 0.35 of a year, and they are also superior to the boys in every other respect, as follows: intelligence quotient, 4.28; reading G-scores, 0.93 (one whole grade); arithmetic G-score, 0.08; combined arithmetic and reading G-score, 0.65 (two-thirds of a grade); and teachers' rating G-score, 0.50 (half a grade). When the same comparisons are made between the dextral boys and girls, it is found that the girls are younger than the boys by 0.12 of a year, and are in addition superior to the boys as follows: intelligence quotient, 0.41; arithmetic G-score, 0.09; and teachers' rating G-score, 0.88; but they are inferior to the boys in reading G-score, 0.10; and in combined arithmetic and reading G-score.

In this connection it may also be of interest to compare dextral girls with sinistral girls and dextral boys with sinistral boys. Sinistral girls are older by 0.21 of a year than dextral girls and are lower in intelligence quotient by 0.98 of a point, and inferior in arithmetic G-score, 0.23 (one-fourth of a grade); but they are superior in reading G-score, 0.44 (one-half of a grade), and in teachers' rating G-score, 0.08. The corresponding comparisons between dextral boys and sinistral boys reveal the following: the left-handed boys are older by 0.28 of a year; inferior in intelligence quotient, 4.85; in reading G-score, 0.69 (two-thirds of a year); in arithmetic G-score, 0.21; and in teachers' rating G-score, 0.37; in other words, in every respect. It would appear that sinistrality on the whole affects girls slightly less than it does boys.

Summary

The results of this study may be summarized somewhat as follows.

(1) In a preliminary survey the following data were obtained: (a) Among 2328 junior high school pupils, 4.62% of the boys and 2.62% of the girls were left-handed. (b) As to change of handedness, presumably as a result of social pressure, 5.23% of the boys and 4.14% of the girls had been changed from left-handed writers. (c) The percentage of changed handedness combined with the percentage of left-handedness was 9.85 for the boys and 6.76 for the girls. (d) In a comparison of handedness in special rooms and in regular rooms it was found that in 34 regular rooms the percentage of left-handedness was 3.54, while in 10 special rooms for retarded and subnormal children the percentage was 6.37. (e) In classes which had been sectioned on the basis of ability, the percentage of sinistrals in the high sections was 3.51 and in the low sections 3.70. The percentage of changed handedness in the high sections was 3.09 and in the low sections 6.34. The combined sinistrals and changed sinistrals gave percentages of 6.60 for the high sections and 10.04 in the low sections.

(2) The main body of this study was concerned with the relation of handedness to ability in 975 children of the Oklahoma City Schools who were ready to be promoted to the seventh grade. These results may be summarized briefly as follows: (a) When intelligence and mean achievement of the sinistrals and dextrals were compared, it was found that the dextrals were slightly superior in every respect. (b) When the two groups had been equated in intelligence the relative superiority of the dextrals in achievement remained approximately the the same. (c) When handedness was correlated with intelligence, achievement, and chronological age, a superiority of the dextrals was shown to be consistent throughout, although the coefficients were very low. (d) Sinistral girls were slightly superior to sinistral boys in every respect.

Conclusions

From these results the following general conclusions may be drawn.

(1) For the groups of dextrals and sinistrals under consideration, there is a universal superiority of the dextrals in intelligence, achievement, and teachers' ratings. This superiority is slight, however.

(2) Sinistral girls are slightly superior to sinistral boys.

(3) Whether these differences are sufficient to justify the segregation of sinistrals for the purpose of instruction where the facilities for such segregation are available is problematical. It is the opinion of the writers, however, that they are not sufficiently significant to justify segregation.

A CASE OF SOLAR BLINDNESS

By A. B. Crawford and E. M. Ligon, Yale University

The following article discusses a laboratory investigation of the field, limits, and degrees of sensitivity in a paracentral scotoma of solar origin in the left eye; with an analysis of color vision in and about the affected area.

The condition described has remained practically unchanged since its appearance twelve years ago, following a severe exposure of the retina to direct sunlight.

At the time when this injury occurred the subject (*W*) was 27 years of age and serving as an officer in the United States Navy. Vision with his left eye had always been unusually acute. In fact, due to a slight congenital astigmatism of the right eye, *W* had come to depend especially upon the left eye whenever close or very accurate sight was required. Practically complete correction of the deficiency in the right eye was obtained by glasses which, however, were worn only for reading purposes. *W* experienced no difficulty in passing any of the normal series of physical examinations for enrollment in the Navy, or for temporary commission following training at the Naval Academy at Annapolis. Although never able to obtain depth-perception with a stereoscope, or to secure with binoculars a properly fused field of vision, he possesses a sense of perspective which seems perfectly normal as judged by any ordinary criterion. This in itself is an indication that depth-perception and perspective may be developed monocularly as habit-acquirements.

Conditions producing the scotoma. In February, 1918, *W* was ordered to duty in the West Indies. The glare of the sun upon the waters in Southern latitudes, particularly in the spring and summer, is severe; but *W* initially experienced no more discomfort therefrom than did his fellow officers. None of the group wore dark glasses or other glare-protective devices. In May *W* was put in charge of training submarine lookouts. This duty involved, for some two hours each afternoon, the sighting and identifying of various objects, frequently as seen against the sun or its reflection.

After about a month of this special duty, in addition to his regular Watch Officer routine, *W* awoke one morning (without having previously experienced any symptoms of discomfort as forewarnings) to find himself apparently blind. A purplish cloud, similar in color to the after-image usually caused by looking at the sun, seemed to occupy the whole field of vision. After some hours the apparent after-image (the shape of which did not recall any particular primary image) had reduced materially in extent, though it still remained sufficiently intense in color and opacity to obliterate all objects in its immediate field.

Subtending a vertical angle from the fixation point upwards of about 10°, and extending for a greater distance on either side, the outline of the scotoma was then roughly rectangular. In the succeeding twelve years it has not altered materially in size or shape, so far as *W* is aware, though it has markedly faded

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in apparent saturation. Also, as will be more fully discussed later, it is now less opaque in certain areas than it was originally. Though only the left eye was affected, *W* initially 'saw' the scotoma continually; he perceives it now only upon closing the right eye.

Upon return to office work at Yale University after discharge from service, *W* began to suffer severely from headaches, had attacks of diplopia and after six months began to develop incidental nervous disorders. Sleep, digestion, and cardiac action were all functionally affected, though no organic disturbance other than that in the left retina could be discovered.

Dr. William H. Wilmer, then of Washington, was consulted and diagnosed the condition as "Solar Blindness produced by retinal lesion near the macula, with probably permanent damage to retinal cells as result of ultra-violet activity." He prescribed complete rest for six months from any close ocular work, and the use of dark glasses (Crookes B toric lenses). At the conclusion of this period *W* returned to his work. His eyes are still acutely sensitive to glare, and particularly to unprotected artificial light. *W* avoids taxing them unduly for prolonged periods, as their continued close use causes headaches, feelings of muscular strain, and occasional diplopia. Although the scotoma persists, adaptation to it has been sufficiently well established for him to carry on office work, reading, etc., with regularity, though not without strain and inconvenience.

Experimental investigation of the scotoma. The procedure of investigating and mapping the affected area was, after accurately determining the point of fixation, to move a stimulus-spot, affixed to the end of a flat bar, from well outside the blind field inwards, until *W* reported its disappearance; the movement being continued in the same direction until the stimulus reappeared. The various reports were noted by the experimenter in terms of distance and direction from the fixation-point. *E* was screened from *W*'s vision.

Literature descriptive of analogous conditions indicated that, for the purpose of this investigation, the tangent screen method was superior to the use of any form of perimeter, even the most modern and elaborate type such as the Ferree-Rand.[1] A certain amount of distortion takes place when projecting retinal images upon a plane surface, but this is relatively small in the case of a central scotoma and does not materially affect the particular problem. Therefore a procedure similar to that usually adopted in plotting the normal blind spot was followed.

Accurate mapping and studying of retinal areas obviously necessitates careful control of stimulation, fixation, and illumination. The experiment was therefore conducted in a laboratory dark-room, using the following equipment:

(1) A tangent screen, 2 m. square, mounted on a heavy standard, with its face covered with heavy gray cardboard corresponding closely to Hering Gray No. 21. (2) Judd head-rest, with overhead screen, eye-shield and mouth-bit, and chair. (3) "Daylite" Roundel reflector and filter (Macbeth, Corning) containing 100-W Mazda Daylite bulb mounted behind and above the overhead screen.[2]

[1] C. E. Ferree and G. Rand, An illuminated perimeter with campimeter features, *Amer. J. Ophth.*, 5, 1922, 455-465; cf. also L. C. Peter, *The Principles and Practices of Perimetry*, 1923.

[2] For the use of daylight illumination see A. J. Brown, Some uses of artificial daylight in the psychological laboratory, this JOURNAL, 27, 1916, 427 ff.; and G. J. Rich, The daylight mazda lamp in the psychological laboratory, *ibid.*, 30, 1919, 313 ff.

(4) Series of stimulus-objects attachable to a specially constructed rod, and standard Hering color papers. (5) Fixation point monocularly fixated and found by the use of a fixation-mirror. (*W*'s right eye was shielded throughout.)

The close approximation to daylight afforded by this equipment and the uniformity of illumination so obtained at all times, satisfactorily met the experimental needs. Use of transmitted monochromatic stimuli, equated as to brightness and apparent saturation, would have been superior to reflected light from colored papers, but the investigation did not then seem to warrant such refinement of technique. The most serious uncontrolled factor was the resulting difference in intensity of the stimuli.

The criterion taken for determining the outline of the field of absolute scotoma was complete disappearance of the stimulus-object. The adjoining area of partial sensitivity yielded a graded subjective response shading off from bare perception (on emergence from the field of complete scotoma) to normal vision. The exact outline of this partially affected area is therefore difficult to determine and is probably a function in part of the intensity and saturation of the stimulus used.

Great care was taken in determining and maintaining the fixation-point. First *W*'s orientation was adjusted by use of the head-rest and mouth-bit; then the distance from cornea to plotting surface of screen was set at exactly one meter and the screen adjusted at right angles to the line of vision. The point of fixation was determined by the mirror, and the reflection of *W*'s pupil in it was taken as the central origin.

To prevent its extension into the blind field, a T-shaped figure, or cross minus its upper vertical arm, was employed as a fixation-object. This figure, cut from white paper and located by the mirror had three arms each 5 mm. broad and, from its center, 10 mm. long. Inaccurate fixation and straying vision produced a readily noticeable after-image outlined against the gray of the screen surface and thus was at once noticeable to *W* and readily correctible.

Stimulation was initially effected by white and colored pinheads 2 mm. in diameter, affixed to the end of a flat stimulus-rod, faced with gray cardboard similar to that on the surface of the tangent screen. Stimulus-objects of 1 cm. in diam., cut from standard laboratory papers (Hering red, green, blue, yellow, and white) were later used for plotting the normal color zones, as the smaller stimulus proved impractical for testing peripheral color vision. Use of the smaller stimulus-object for determining extent of the scotoma proper, however, greatly facilitated accuracy in recording the outline of that area.

In order to secure quantitative and readily measurable data, light radial lines (imperceptible to *W*) were ruled on the tangent screen 10° apart, which facilitated subsequent reduction of the plotted outlines to a smaller scale. Readings were repeated on successive days and were sufficiently consistent, especially as expressed in terms of the arc subtended by the limits of error, to indicate high reliability and close control of possible factors of error.

Approximately 30 individual readings were made for each of the 10° sectors passing through the affected area. The smallest mean variation from the average of readings for any single 10° sector (1.5 mm.) was on the minimal diameter of the scotoma (at 100°) and the largest variation (4.2 mm.) on the largest diameter (at 150°). The average of these variations for each sector was only 2.6 mm. for a total of 448 readings (the equivalent of approximately 0.041 mm. on the surface

of the retina). The fovea is said to subtend an angle of from 55′ to 70′ of arc,[3] and Ladd and Woodworth accordingly infer that fixation,[4] even under the most favorable conditions, involves a possible error, attributable to the size of the fovea, of at least 1°. Therefore fixation seems to have been well controlled.

Results. The results obtained were initially analyzed, for all stimuli employed, in both tabular and graphic forms. Two of the graphs only, however, are here reproduced. The first of these outlines only the affected area, as determined by the use of both white and yellow stimulus-objects, 2 mm. in diameter. The second plot represents responses to green and yellow stimulus-objects each 1 cm.

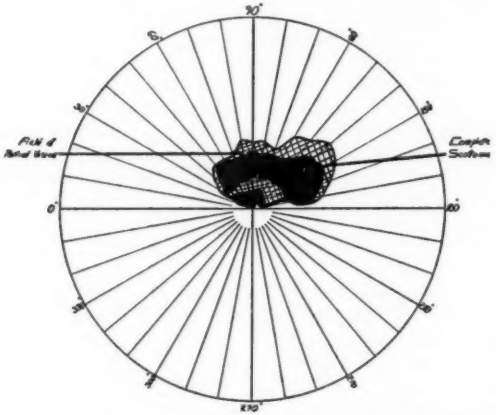

FIG. 1. OUTLINE OF SCOTOMA FOUND BY USING WHITE AND YELLOW STIMULUS-OBJECTS TWO MM. IN DIAMETER

in diameter. The solid black denotes complete scotoma; cross-hatching, adjacent, variably opaque but not wholly blind areas of partial vision (achromatic and indistinct); and stippling, the general region where 'color islands' appear in the otherwise achromatic fields. The small unshaded area indicates the extent of normal chromatic response to green; the larger unshaded area the corresponding response to yellow.

Color vision in general outside of the affected area was found to be normal. Since tints and saturations were not equated, the data as to relative extension of these color fields are without significance.

Within the area of achromatic vision, red and blue stimuli are visible to *W* as dark gray (almost black); yellow and green as light gray (almost white). The size of the stimulus spot and the intensity and saturation of the color affect the apparent extent of this field to a degree not quantitatively determined.

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[3] J. Parsons, *Introduction to the Study of Color Vision*, 1915, 10.

[4] G. T. Ladd and R. S. Woodworth, *Elements of Physiological Psychology*, 1911, 459.

The entire field of color vision *above* the scotoma shows a peculiar 'spotty' response to all colored stimulation. Fluctuation between the proper color and gray was reported by W, when a 2-mm. stimulus was moved through the upper field, as if occasional islands of color perception alternated with achromatic areas. With the larger stimulus-object this phenomenon did not appear.

These foci of color-sensitivity, revealed by the use of the small stimuli, moreover proved too minute to be accurately plotted. Probably the small irreducible fixation-error caused these 'islands' of color sensitivity to shift their apparent localities. As the retinal projection of the small stimulus would have a diameter

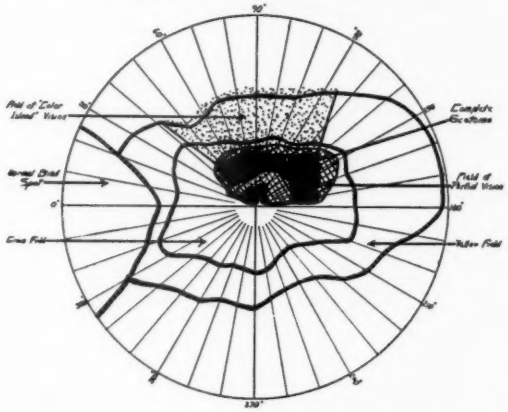

FIG. 2. OUTLINE OF GREEN AND YELLOW COLOR-FIELDS AND SCOTOMA FOUND BY USING GREEN AND YELLOW STIMULUS SPOTS ONE CM. IN DIAMETER

about ten times greater than that of a single cone, no reliable inference may be made as to the physiological basis for this fluctuating response.

Certain of the results may warrant further and more detailed investigation, because of their possible bearing upon evolutionary theory. For example, if the entire retina, in an earlier stage of its development, was supplied only with such mechanisms as those to which the periphery is still limited, and the vicinity of the fovea subsequently evolved a higher form of mechanism, it is conceivable that the latter would also be more delicate and more susceptible to injury than the coarser equipment of earlier origin. In the case of W, accordingly, ultraviolet activity may have been sufficiently intense over that portion of the retina where the scotoma is complete to have damaged the visual mechanism there entirely and permanently; whereas adjacent areas (which now yield a typically peripheral response, despite their proximity to the fovea) may have been subjected to slightly less severe solar activity. On the fringe of the now wholly blind field, therefore, this may have affected chiefly the mechanism for particularly accurate and chromatic vision, leaving the hardier structures in that area relatively unimpaired.

Summary. *W*'s paracentral scotoma, of solar origin, was plotted under conditions sufficiently controlled as to illumination and fixation to justify the following conclusions.

Part of the retina was so damaged as to make it completely insensitive to normally adequate stimuli.

Part of the paracentral retina now responds to stimulation much as does the periphery of the normal retina, *i.e.* with varying degrees of clearness and with no color discrimination.

Part of the upper visual field shows an irregular response to color stimuli, small foci of normal sensitivity being interspersed with larger areas which respond only achromatically.

The entire lower half of the field and part of the upper half are entirely normal.

APPARATUS

AN INSTRUMENT FOR TESTING THE LIGHT AND COLOR SENSE WITH IMPORTANT FEATURES OF STANDARDIZATION AND CONTROL

By C. E. Ferree and Gertrude Rand, Johns Hopkins Medical School

The instrument for testing light and color sense,[1] to be described here, has the following features and advantages. (1) It is easily portable, quick and convenient to operate, and gives a high reproducibility of result.

(2) The amount of light entering the pupil of the eye, the density of light in the test field, or the brightness of the test field can be measured and the instrument calibrated in terms of any or all of these measurements. Differing from any other instrument that has as yet been described, however, all the light in the test field enters the eye. In comparing the results of this instrument with those obtained with any other type of instrument, this fact has to be taken into account.

(3) The results are independent of changes in the size of the pupil, including the changes produced by the use of pilocarpine and other myotics.

(4) Very effective and convenient means for the control of fixation are provided.

(5) The instrument seems to be approximately independent of the condition of refraction of the eye. This permits of a more direct examination of the powers of the retina than is ordinarily the case and adds greatly to the ease and convenience of making the examination. It also decreases the range of difference or the scatter of results in a group of observers. This is a very important feature in the use of the instrument for diagnosis.

(6) Because of the small aperture in the eyepiece the results are further rendered independent of the influence of distance of projection of the image. In monocular vision the distance is ordinarily dependent on the amount of accommodation. The distance of projection of the image and its resultant subjective magnification have a small but appreciable effect on sensitivity.

(7) The size of the test field may be varied through a visual angle ranging from 36° to 0°. These changes may be quickly and conveniently made. The test field may also be made to have any shape desired. Just what size of field should be used for diagnostic work has not yet been determined. Points to be considered in making the selection are: (a) reproducibility of result; (b) the range of scatter of results for normal observers; (c) diagnostic sensitivity; and (d) control of fixation. In connection with (d) it may be pointed out that a large field renders the results less liable to the variability that is caused by insufficient control of fixation or small residual changes of fixation which defy control. Because of these facts it is of importance for the present at least that the instru-

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ment should have considerable flexibility as to possibilities of field size. Range of size is needed too if the test field is to be used also as preëxposure field in determinations of the adaptation curve.

(8) Provision is made such that the broken circle may be used as test object in which case an objective check is had on the correctness of the judgment and an additional very effective control of fixation is secured. It can also be used under conditions which test acuity at low illumination, or in fact at any level of illumination desired with the eye, as already stated, rendered approximately independent of errors of refraction. The ability to render the eye independent of condition of refraction, it is scarcely necessary to note, is a very important feature in testing both the retina's power to sense light and its power to discriminate detail. With this instrument the light and space sense can be tested under conditions which apparently render the test approximately independent of the resolving power of the refracting system of the eye.

(9) A wide range of intensity of light is provided under quick and convenient control. The change of intensity is produced without change in the color of the light. The spectrum composition and color temperature of the light can be specified if desired.

(10) To insure the constancy of the efficiency of the lamp a checking standard is furnished such that at any time the illumination of the test field for a given reading of the scale can be compared with that given by a standard lamp and a corrective change made in the operating current if needed.

(11) The instrument can be used with equal facility in any state of adaptation, light or dark.

(12) All scales can be read by the examiner in the dark and all adjustments of the instrument made without interfering with the state of adaptation of the observer. The scales are illuminated by a light from the inside of the instrument. In addition the readings may be recorded by pressing an indicator. The examiner may thus watch the illuminated scale as a guide for his procedure while the test is being made and make a permanent record of the settings when needed at any time during the course of the examination without the introduction of extraneous light. Moreover, the eye is placed at once in position and there is no groping to find the test field. A single determination can be made in a few seconds. The construction of the apparatus and the method of presenting the light to the eye are such as to minimize the inconvenience usually entailed in working in a completely darkened room.

(13) If an adaptation curve is wanted, a preëxposure field is provided which may be varied through a wide range of size and intensity. The instrument also is very conveniently adaptable to the use of any extraneous means of preëxposing the eye to light of the desired intensity.

(14) The instrument can be used for detecting either the light minimum or the light difference. It has been designed, however, primarily for determining the light minimum.

(15) By the insertion of appropriate filters the instrument can be used just as effectively for testing the color sense and the achromatic sensitivity to colored light as for testing the light sense.

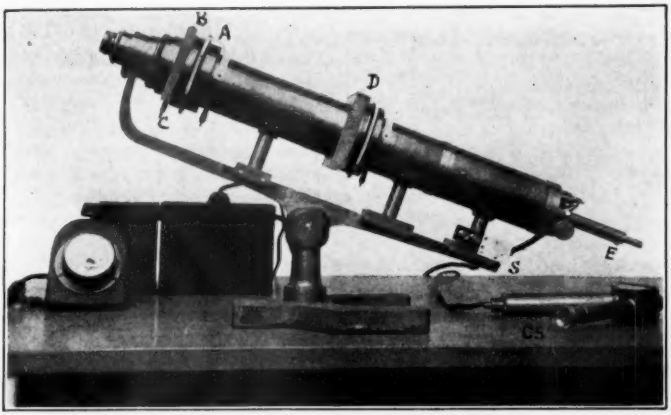

FIG. 1. APPARATUS FOR TESTING LIGHT AND COLOR SENSITIVITY

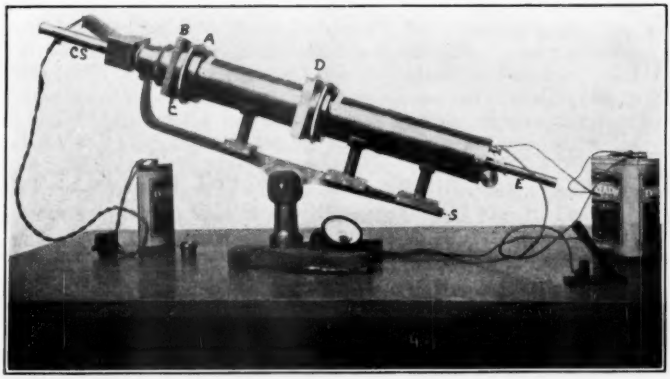

FIG. 2. SHOWING APPARATUS IN POSITION FOR USE
(Double-throw switch connecting both the lamp of checking standard and the test lamp with the ammeter not shown in picture)

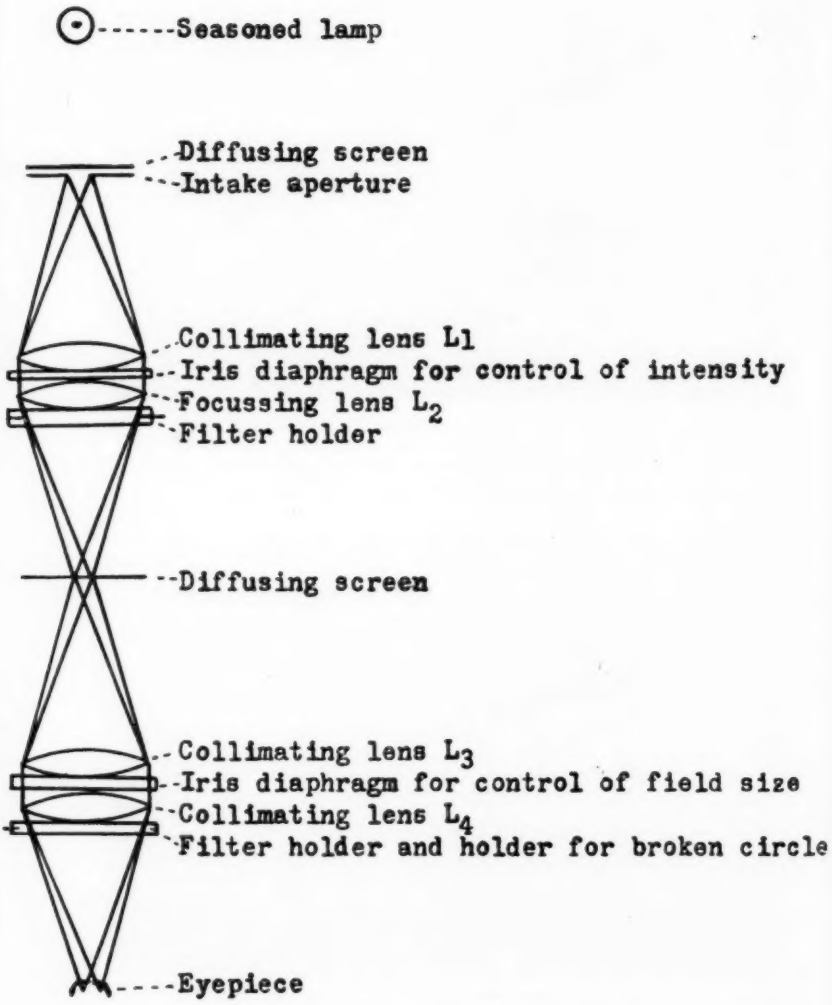

FIG. 3. DIAGRAM OF THE OPTICAL SYSTEM

(16) The instrument can also be used very effectively for the detection of very small central scotomata for either light or color—scotomata of such a size as to be difficult of detection with the perimeter or tangent screen; yet of sufficient importance to reduce acuity.

The general plan of the instrument is as follows. Light from a circular intake, 14 mm. in diam., is collimated and focussed to an image and the light from this image is again collimated and focussed to a second image which is approximately at the nodal point of the refracting system of the eye. The intake is covered with a plate of diffusing glass and its first image is received also on a plate of diffusing glass. The diffusing plates are of advantage in securing an evenly illuminated test field and in producing a variation of intensity by changing the size of aperture of the focussing lens. The intake is illuminated by a small, well-seasoned lamp, the distance of which from the intake can be varied in continuous change. An eye-piece is provided with a viewing aperture 1 mm. in diam. The eyepiece does not contain a lens. Various stops and diaphragms including an iris diaphragm are supplied for changing the size and shape of the test field and for aiding in the variation of the intensity of its illumination. The optical parts are housed in a tube which is light-tight with the exceptions of provision made for the illumination of the reading scales. The instrument is mounted on a U-shaped base and is provided with a tilting adjustment for height.

A picture of the instrument is shown in Figs. 1 and 2, a diagram of the optical system in Fig. 3. The optical system is made up of two sets of collimating and focussing lenses, L_1 and L_2, L_3 and L_4, each 65 mm. in diam. The second of these sets of lenses, L_3 and L_4, the one nearest the eye, furnishes the test field. The principle is that when the nodal point of the refracting system of the eye is placed at the principal focus of a lens which receives parallel rays of light, the lens appears to be filled solidly and uniformly with light. Thus a very satisfactory test field is provided and all the light from this field enters the pupil of the eye. This latter feature is of very great advantage in the calibration of the instrument inasmuch as the amount of light entering the eye can be measured. Also it is conservative of light in case high intensities are wanted as preëxposure for the eye, as may be needed, for example, when an adaptation curve is to be determined. In accord with this plan, therefore, lens L_3 collimates or renders parallel the light rays for the test field and lens L_4 focusses them on the pupil of the eye. All of the lenses are double convex and of a strength of 10 diopters. There is thus no magnification or minification in the system.

The following provisions are made for changes in size and shape of field. (1) At A near the back surface of the focussing lens L_4 and between the two lenses L_3 and L_4, is mounted an iris diaphragm with a range of opening from 5–65 mm. At a distance of 10 cm. from the eye this gives a field ranging in visual angle from 2.87°–36°. (2) A grooved cell or holder is located at B immediately in front of the focussing lens in which a diaphragm of any size or shape may be inserted within the range of the size of the lens. One of the devices provided for insertion in this cell is a broken circle. This circle may be made of any size within the limits noted above. Three advantages may be assigned for the use of a broken circle here: (a) by turning its opening into different positions an objective check may be had on the judgment—*i.e.* the observer may be asked to indicate the direction in which the opening points; (b) its use provides an ex-

cellent control of fixation,—*i.e.* the discrimination of a form detail, the opening in the circle, requires that a clear image be obtained and that this image be located on the fovea; and (c) depending upon the size of this opening, the test may be made to vary in type through the range of an acuity test at low illumination at one extreme to a light sense test at the other,—*i.e.* objects are seen because of their size and their sensation difference from the background. When the difference from the background is large and the detail to be discriminated is of threshold size, we have what is called an acuity test. On the other hand when the detail to be discriminated is large and simple in form, and the difference from the background is of threshold value, we have the essentials of the conventional light sense test.

A test for acuity at low illumination doubtless should be added to our diagnostic program. The following are some of its features. (a) The control of fixation is excellent. The discrimination of a threshold detail requires exact fixation and supplies its own check on the accuracy of the accomplishment. (b) By decreasing the sensation difference between object and background through decreasing the intensity of illumination and by requiring the discrimination of detail at the level of sensation difference chosen, the test combines the features of an acuity test with those of a light sense test which may be made, if desired, at a very low level of light adaptation. And (c) a test for acuity at low illumination is quicker and much more easy to make than a threshold test of the light sense, *i.e.* the annoyances and difficulties of working in a completely darkened room and with complete dark adaptation are avoided. As already pointed out, however, the construction of the instrument and the method of delivering the light to the eye are such as to minimize the difficulties usually encountered in making a test of the light sense in a dark room.

In all probability there is a diagnostic advantage in being able to reach the lower end of the adaptation scale. This can be accomplished in case of the present instrument by using a circular, square or other simple form of field of appropriate size. Or if it is wanted to use the broken circle, very low levels of adaptation can be reached and still enough of the acuity feature be preserved to serve as an objective check on the judgment and as a very substantial aid in the control of fixation by using a circle with a larger opening than accords with the standard scale. The circles used with this instrument are stencilled in very thin plates of hard sheet aluminum.

The cell which holds the stops or diaphragms is also made to hold a filter in an adjoining groove. The bottom of the cell is provided with a plunger, shown at C, Fig. 1, to lift the filter for easy and convenient removal. In order to provide for the rotation of the broken circle when desired, the cell is contained in an independent mount which turns freely. On this mount are 8 stops which indicate the direction which the opening is wanted to have—right, left, up, down, and the four intermediate 45° positions. For convenience of rotating the holder a small knob is placed on one side of the holder in line with one of the stops. The position of the knob can be located by touch in the dark. The plate containing the broken circle can be inserted with the opening in any one of four relations to the knob (right, left, up, or down) that may be desired—which relation is known to the examiner but not to the subject. The examiner thus is always aware of the position of the opening of the circle and able to check the correctness of the sub-

ject's judgment. The stops are provided as follows. The back wall of the collar of the holder contains 8 very small shallow cups in the 8 positions which the opening of the circle is wanted to have. In the wall of the collar of the stationary mount for lenses 3 and 4 and the iris diaphragm immediately back of the holder for the broken circle, a small ball is sunk against a spring, to give it tension, at the proper level to engage the cups as they pass in rotation. The top of the cell is closed by a narrow slide to hold the filters in position during rotation.

The test field presented is viewed through an eyepiece with its aperture at a distance slightly less than the focal length of the field lens. The front of this eyepiece is concaved so as closely to fit the front of the eye. The aperture in the eyepiece is 1 mm. in diam. It was made of this size in order that it might be smaller than the pupil of any eye that might be examined, even when under the influence of pilocarpine or other myotic. The results of the test are thus safeguarded against all variation due to changes in size of pupil. Also an aperture as small as this when placed in close contact with the eye renders the observation practically independent of the refracting system of the eye. The broken circle viewed at its distance of 10 cm. from the aperture can be seen clearly, for example, by high hyperopes and presbyopes and is seen equally clearly in any position in which it may be turned by eyes having a high astigmatism. Indeed with this eyepiece held in position in front of the eye, there is apparently no clearly defined near point, *i.e.* sufficiently clear vision is present as near as the object can be brought to the eye for the discrimination of the details of small print. In this connection it may be noted that artificial pupils are usually placed at too great a distance from the eye for satisfactory results.

The lens system located farther from the eye and next to the light intake is designed for the control of intensity. It also is made up of two 10-diopter lenses, L_1 and L_2, one for collimating the light from the intake, the other for focussing it. Between these two lenses is an iris diaphragm, range 65 to 5 mm., which is used to decrease the volume of light focussed into the image. That is, the purpose of this lens system with its appended iris diaphragm is to vary the intensity of light which enters the eye; the purpose of the lens system and iris previously described is to present a test field whose size can be varied independently of the changes in the aperture of the lenses which are used to vary the intensity of light. At D immediately in front of the lens system is a double cell for the reception of filters, neutral or colored as may be desired. For their convenient insertion and removal the filters are held in a double container provided with a nib or grip at the top which can be easily located in the dark. This holder is lifted up for the removal or insertion of filters.

The iris diaphragm for each lens system is provided with a slender pointer which moves over a transilluminated graduated scale. The light illuminating this scale comes from the inside of the tube. In order that the illumination of these scales might be independent of the light illuminating the test field, a small (grain of wheat) lamp is set in the opposite wall of the tube directly beneath each scale. These lamps are on the same circuit and can be turned on or off at will by a small switch shown at S. The instrument is calibrated in terms of the graduations on these scales. In one case the calibration is in terms of size or visual angle of test field and in the other in terms of photometric intensity. The photo-

metric calibration may be in lumens, foot or meter-candles, or lamberts. The most suitable unit for calibration of the amount of light entering the eye is the lumen, the unit of emission or quantity of light. The lumen may be defined as the amount of light emitted in a unit solid angle (steradian) from a source of 1 candlepower. The calibration would probably be in terms of milli-lumens, or some smaller subdivision arbitrarily selected. The relation of the lumen, the photometric unit of amount or quantity of light, to the foot-candle and the meter-candle, the units of density of light on a surface or of illumination, may be expressed as follows. A surface of one square foot receiving one lumen of light on a square foot is said to have an intensity of illumination of one foot-candle; a surface receiving one lumen on a square meter, a meter-candle of illumination. If a calibration is to be made in terms of density of light in the test field, then the units used will be the foot-candle, the meter-candle, the kilometer-candle, or one of their subdivisions. If the calibration is to be made in terms of brightness, the units available are candle-power per square inch, candle-power per square centimeter, candle-power per square meter or square kilometer, the lambert and its subdivisions. The brightness units were devised to express candle-equivalents of illuminated or luminous surfaces. The lambert may be defined as the brightness of a perfectly diffusing surface emitting one lumen per square centimeter. Subdivisions are the millilambert and the micro-millilambert. The terms candle-power per square inch, candle-power per square meter and candle-power per square kilometer are self-explanatory.

As an additional means of making small changes of intensity, particularly essential near the threshold of sensation, the distance of the lamp from the intake of the optical system can be varied through a satisfactory range by means of a rack and pinion adjustment. The lamp is mounted at the end of a rod which passes through an opening in the diaphragm at the end of the instrument farthest from the eye. This rod, E, carries a graduated scale which passes under a fixed pointer. This scale can also be calibrated in terms of any of the units mentioned above. It too is illuminated by light from the inside of the tube. That is, a small bit of mirror (polished metal) located just above the pointer reflects the light directly on the scale from a small slit in the diaphragm closing the end of the tube. The instrument is thus self-contained as to devices needed for reading its various adjustments. No extraneous sources of illumination are needed.

For the control of intensity, three means are thus provided: the use of neutral filters for the gross control, the iris diaphragm, and the distance of the lamp from the intake for the finer control. In the use of the instrument for the simple detection of the threshold it will probably be found of advantage to provide an upper limit of intensity by the use of a filter and a given setting of the iris diaphragm, and to vary the light towards the threshold through the range thus provided, by changing the distance of the lamp with the rack and pinion adjustment. For the determination of adaptation curves and the use of the test field as pre-exposure, however, a greater range and flexibility of control of intensity is needed. This is amply provided for by the utilization of all three types of control.

Provisions are made also for making a record of the settings of the instrument during the course of the examination without the introduction of light into the room. These provisions are as follows. (1) The adjustment of the iris diaphragms.

On the outer wall of the tube of the instrument just in front of the diaphragm, a substitute holder is made into which paper scales may be easily inserted and fastened. The graduations of these scales duplicate those on the illuminated scales. Fastened to the milled ring which operates the iris diaphragm is an indicator with a sharp punch which passes over the scale as the milled ring is turned. When this indicator is pressed the graduation on this scale is punched which corresponds to the graduation underneath the pointer on the illuminated scale. At the end of the examination the paper scale is removed and a fresh one inserted. By making the punch a pencil point, the dot can be erased and the same scale used for several examinations if desired. (2) The adjustment of the distance control. In case of this control the rod on the end of which is the lamp illuminating the intake, passes through a tube. At a suitable place near the lamp-end, this rod is planed off to give a surface for graduation. For the viewing of these graduations a slot of suitable dimensions is cut in the enclosing tube. As the rod is moved back and forth the graduations pass under a pointer fastened to the tube at one side of the slit. These graduations, illuminated as described earlier in the paper, serve as a guide to the procedure during the course of the examination. At the farther end of the tube a holder is mounted into which a paper scale graduated in duplicate with the illuminated scale may be inserted. At the end of the rod is fastened an indicator at the end of which is a punch which travels over the graduations on the paper scale. When this indicator is pressed, the scale division is punched which corresponds to the division under the pointer on the illuminated scale.

The checking standard, CS, is also shown in Figs. 1 and 2. In Fig. 2 it is shown in position for use. It comprises a photometric field, an illuminating tube with a standard lamp, and an eyepiece. In using this standard, the eyepiece of the light sense tester is removed and the short receiving tube of the checking standard is inserted in its place. When in this position the image from the test field formed by the light sense tester is received on the inner half of the photometric field and the light from the illuminating tube on its outer half. With the rheostat in circuit with the light sense tester set to give the same reading of the ammeter as was used in the calibration of the instrument and with a given setting of the standard lamp and reading of ammeter, the light is varied in the test field until the half of the photometric field receiving the test image matches in brightness the half illuminated by the standard lamp. The two readings of the ammeter and the reading of the intensity scale of the test field and of the distance scale of the standard lamp constitute the data needed for the use of the checking standard. At any future time with adjustments made in accord with these readings, the two halves of the photometric field should match unless there has been a change in the efficiency of the test lamp. If there has been a change, the amperage of the test lamp is varied by means of the resistance in its circuit until a match is again obtained. The experiments are then conducted with the lamp operated at this amperage. When the test lamp has become old and its efficiency varies rapidly, it should be discarded and a new lamp used. To determine the correct amperage for the operation of the new lamp the checking standard is again used. A quick means is thus provided for checking the efficiency of any lamp and of insuring at all times that the instrument is giving the intensities represented in its calibration. The lamp of the checking standard can be expected to remain constant almost indefinitely since it is operated for only a few seconds at a time.

In order to give ease and precision to the comparison of the brightness of the test and checking field, the light illuminating the two fields should be closely similar in color. This is accomplished by using the same type and wattage of lamp and the same kind of diffusing screens in the checking standard as are used in the instrument. Low voltage lamps operated on dry cells can be used, as shown in Fig. 2, or if preferred, low wattage lamps operated on the lighting circuit. If the current is taken from the lighting circuit a voltage regulator, shown in Fig. 1, should be used to give constancy of current. Small portable regulators can be obtained which give sufficient constancy of current for the purpose of the test.

It is obvious that used in connection with the checking standard the instrument can be employed quite effectively for determining the light difference as well as the light minimum. There can be no better and more sensitive field for determining the light difference, for example, than the type of photometric field contained in the checking standard, the inner half of which can be varied minutely in brightness from the outer half and the two fields so arranged as to give the maximum of contrast; when so used, however, the lamp of the checking standard should be replaced by another lamp, *i.e.* the standard lamp should have as little use as possible. With the distance of the lamp adjustable as it is in the checking standard, the light difference may be obtained at different levels of intensity through a wide range of intensities. It seems probable, however, that the light difference has its maximum of diagnostic significance at very low levels of intensity—levels which lie close to the light minimum. More importantly, perhaps, than the sensitivity to light, the power to change this sensitivity, namely, the adaptation process, is affected by pathological conditions. In any event the adaptation process should not be left out of account in the formulation of our test procedure.

By the insertion of the appropriate color filters the instrument can be used as effectively for testing the color sense and the achromatic sensitivity to colored light as for testing the light sense. The work which we have done thus far on pathological cases in perimetry, scotometry, and blind-spot determinations has convinced us that in the paracentral and peripheral field the testing of the color sense far surpasses the testing of the light sense in diagnostic sensitivity. There seems good reason to suspect that this may also be true of testing in the central field. The specification of the results of testing the color sense presents difficulties of course due to the lack of units for measuring color. We hope, however, to devise a means for making a specification that will be satisfactory within practical limits. It certainly can be done as satisfactorily for work in the central field as it is for the work in the paracentral and peripheral field in perimetry, scotometry and blind-spot testing.

We find that the instrument can also be used to great advantage in testing for central scotomata which are so small as to escape detection in work on the tangent screen. These scotomata are indicated by a reduction of acuity which can not be remedied by any correction of refraction. By using a broken circle as test object with an opening so small as to be near the threshold of acuity, the patient with such a scotoma fails to detect the opening. The control of intensity provided in the instrument is a valuable feature in enabling the detection of defects representing different degrees of depression of sensitivity. The test can be made for a

scotoma for color by covering the opening in the test object with a small bit of gelatine filter. That the failure to detect the opening is not due to an uncorrected error of refraction is ensured by the small aperture in the eyepiece (1 mm. in diam.) which as already stated when placed close to the eye renders the eye approximately independent of refraction.

A FLEXIBLE FORM OF THE CARR SLOT-MAZE

By Coleman R. Griffith, University of Illinois

The Carr slot-maze has recommended itself to studies on a good many types of learning because the exact pathway is hidden from the subject. If the paths are laid at right angles to one another and of a unit length, the whole maze will present a homogeneous field to the subject who must then free himself still further from visual clues if he is to learn the 'correct' path. In a series of studies

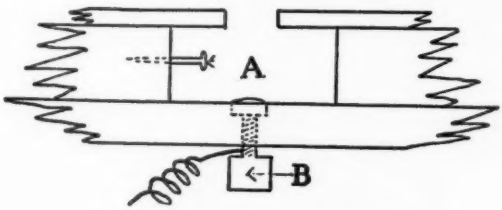

Fig. 4. Cross-Section of Slot
Showing peg at A, and contact point at B

on the relation between rate of movement and form of movement, it was necessary to secure still greater flexibility in the use of the maze and we have, therefore, worked out a piece of apparatus with the following plan. The entire maze is 118 x 60 cm. The foundation of the maze is a hardwood, laminated surface approximately 2 cm. thick. Upon this hardwood foundation there is a single sheet of hard rubber 2 mm. thick. (Bakelite will serve just as well.) This surface of hard rubber is the surface over which the stylus runs, and since it is a non-conductor it makes possible the insertion of the contact points to be described below.

The slot-maze, it will be recalled, is formed by laying out a series of pathways in such form that the lower part of the stylus used in the maze is much wider than the upper part, as is shown in Fig. 4. Pegs (A, Fig. 4) along the side walls of the concealed portions mark blind alleys. In our maze, the width of the lower part of the maze has been increased to 2.5 cm., while the upper part of the path, *i.e.* the path visible to the subject, is 1 cm. in width. The lower part of the paths is formed by parallel strips of wood 1 cm. thick and 2.5 cm. wide. Upon these strips of wood, hard rubber (or bakelite) strips 4 cm. wide can be laid. Since our own maze is 60 cm. wide, it accommodates 11 right-left or left-right pathways. Any

number of openings in the up and down direction can be made. As we have suggested, however, such a maze would serve a good many purposes to still better advantage if all paths were of unit distance and at right angles to one another.

Along the bottom of each pathway and spaced exactly 10 cm. apart are round-headed brass screws (B, Fig. 4) which run through to the back side of the maze where they are soldered to terminals. In our own maze, these contact points and the pathways are so arranged with respect to one another that a contact will

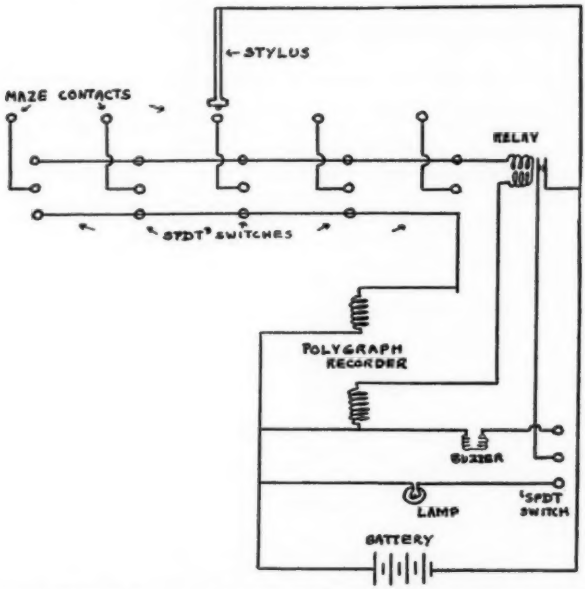

FIG. 5. WIRING DIAGRAM OF THE FLEXIBLE FORM OF THE CARR SLOT-MAZE

normally fall at every turn and at 10 cm. intervals between each turn. Each of the contact points is wired to one pole of a single-pole double-throw ('spdt') switch and the switches are wired through a relay either to a buzzer or to an electric light. They are also wired to a recorder as is shown in Fig. 5. By throwing the switches one way or the other, it is possible to create in a few seconds almost any kind of a pattern on the maze, for the correct pathway may be marked by that series of contacts which give neither a buzz nor a light and the wrong pathways may be marked by the sounding of the buzzer or by the light. Since there will be as many 'spdt' switches as there are contact points in the maze, the switches will, when thrown one way or the other, make up any given pattern in the maze. The stylus consists of a brass disk in the base of which is a ball-bearing which allows the stylus to slide over the contact points easily. The con-

tact points are slightly counter-sunk in the hard rubber so that a minimal amount of friction is created. The handle of the stylus is made of bakelite through the center of which a wire is drawn. This wire closes the circuit to the recorder and to the buzzer or electric light.

The maze may be used lying on the table, but we have found it advantageous to set it up at an angle before the subject. If the maze is tilted at an angle of 30° all parts of it become readily accessible to the subject.

In our own experiments, the following procedures have been useful. The contact points, when connected with the recorder, enable the experimenter to make a study of rate of movement through the maze as well as of form of movement. The fact that every one of the contact points is connected through a switch with a buzzer or electric light makes it possible for any type of change in pattern to be made as fast as one can throw a few switches. In one series of experiments, for example, it was possible to send the subject through the maze in one direction according to one pattern and then, in the meantime, throw the switch so that on the return trip a different pattern was presented.

A TACHISTOSCOPE FOR EXPOSING LARGE AREAS

By J. E. Evans, Iowa State College

The tachistoscope described here and illustrated in Fig. 6 allows large pieces of material like street car signs, or large full pages from magazines to be exposed.

The apparatus consists of a light-proof box 38 in. long, 24 in. wide, and 20 in. high. The dimensions, however, are matters of choice or need. The front end of the box has an aperture 2 in. by 6 in. through which the observations are

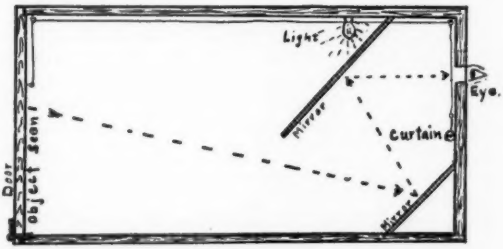

Fig. 6. Tachistoscope for Exposing Large Areas

made. The back end of the box is a door hinged at the bottom. The material to be exposed is attached to the inside of this door by clips or thumb tacks. A small curtain attached by a cord to the door is drawn up before the aperture in front when the door is opened. Closing the door allows the curtain to drop below the aperture.

Two 60 w. daylight bulbs supply the light for the observations. Two perfect plate glass mirrors are set at angles in the box (see Fig. 6) so that the stimulus-

material is reflected when the lights are on into the lower mirror, from there into the upper mirror, and then into the eyes of the observer. The time of exposure may be controlled by connecting the lights with a mercury contact metronome, or similar time-controlling device.

As the observer is able to sit in ordinary light his eyes have little or no need to adapt to the light when the exposure is made.

APPARATUS NOTES

A New Method of Administering Shocks in Animal Experimentation

Most of the experiments in the field of animal learning that call for reward and punishment continue to make use of the electric grill. Now and then attempts have been made to countersink the copper wires or strips which carry the punishing shocks, but even at best the tactual qualities of the punishment area are not equivalent to those of the reward area. Moreover, in many experiments no attempt has been made to countersink the wires or strips, a fact which may make the two parts of a discrimination box quite incomparable. That is, the punishment chamber gives a totally different tactual pattern of stimulus to the subject that is being used in the experiment. Even when gratings are placed in both chambers in order to avoid this criticism, the gratings still remain as an unnecessary part of the whole pattern since rats may be conditioned to a grating, that is, to a tactual pattern even though the shock is not always present.

In a series of studies on the effect of emotional conditions on learning, it was found that this difference between the stimulus-value of paths giving punishment through an electric grill and those affording a reward without the presence of a grill (and sometimes even with the presence of the grill) became one of the determining factors in the learning process. It was necessary, therefore, to find some new way of administering punishment without modifying the tactual stimulus pattern or any other pattern save that afforded by the punishment itself. The following method has been proven successful. If all the paths of a maze, a problem box, or a discrimination box are painted with two or three heavy coats of metallic aluminum paint, an electric current will, under conditions to be named below, distribute itself over the whole film of paint. A white rat placed in a maze covered by such a layer of metallic paint will receive a shock by induction from any and all parts of the maze. The film of paint acts as a condenser and the intensity of the shock will vary with the difference of potential between the film and the body of the rat.

Some of the precise data with respect to this method of administering punishment are as follows. Let us assume that we are using an elevated maze where some of the paths are to offer a reward and others punishment. At the joints there will be a tiny crack which may or may not be filled with the aluminum paint. These tiny cracks offer the opportunity, then, to break the distribution of the electric current throughout the maze. If the experimenter has been careful in the construction of the paths, these cracks may be made so small as to offer small chance of variation in the tactual pattern of stimulus from the maze. Those parts of the total path which are to offer punishment may be wired together in this way. The wires may run along the under side of the elevated path with here and there a staple to keep them in place, or on both sides of the path.

This second method is preferable since the metallic film offers a high resistance to the distribution of the E.M.F. and the wires help, therefore, to reduce this resistance. The wires may be very small, but they *must* be tacked on before the painting is done. If the aluminum paint covers both the staples and the wire, the wire becomes a conduit from one path to another as well as a means of reducing resistance. A maze may be so wired that any given pattern of paths may offer punishment on one occasion and an indifferent situation or rewards on other occasions. The precaution must be taken that the aluminum film does not break away from the wire. This will not happen unless the whole apparatus is given exceptionally hard usage.

A current of 12 v. at the primary posts of a ½-in. coil will, when raised to upwards of 2000 v. in the secondary, be sufficient to yield a good shock. One post of the secondary is left open and the other leads to the wires along the sides of the maze path. Six volts are not enough to give a distinctive shock and more than twelve volts are not advisable although an animal cannot be killed by this method.

Since the voltage in the film is so great, the variation in intensity of shock due to variations in the resistance offered by the feet of the subject is comparatively much smaller than that which commonly prevails where a grid is used and the subject is placed in series with the current. The metallic aluminum paint, when spread over the entire surface of a maze, and broken only by minute cracks, offers a uniform tactual and visual stimulus pattern to the subject of experimentation. The paint can be washed easily after each trial with either water or alcohol without disturbing the surface. It is not slippery and neither does it scratch easily. On the whole, this method offers a quick, economical and easily standardized way of presenting punishment without at the same time changing other stimulus conditions.

University of Illinois. COLEMAN R. GRIFFITH.

A METHOD OF MARKING THE SKIN

One of the most annoying and difficult technical problems in the field of cutaneous sensation is the marking of the sensory spots that are to be punctiformly stimulated over a period of days. I have tried many methods and have found tattooing to be the most satisfactory. The method of tattooing that I use is as follows. A piece of small glass tubing is held over a fish-tail bunsen burner, and is drawn, when it becomes ductile, to capillary size. The tubing is then broken in the capillary section; the fine end is dipped in India ink, wiped dry on the outside, placed upon the spot of the skin (previously sterilized) that is to be marked, and twisted and turned until the point of the capillary tube has bored into the cutis. Then the tube is gently removed, the spot wiped with sterile cotton, and a small piece of adhesive tape placed over it. If care is used, the procedure if aseptic, healing is rapid, and experimenting may begin the following day. Not one of the many spots that I have tattooed by this method has become infected. The size of the spot tattooed is a function of the size of the capillary tube. When very fine capillary sections are used, extreme care must be exercised in twisting and turning the 'drill' and in removing it to keep from breaking the point off in the skin. The pain is hardly noticeable; it is not as intense as a pin prick.

KARL M. DALLENBACH

NOTES AND DISCUSSIONS

Eye-Movements and Visual After-Images

Certain behaviorists tend to dispose of sensory qualities either by attributing them to the environment or by identifying them with a muscular response, or, at different times, by both of these devices.[1] Imaginal qualities have usually either been considered conditioned implicit responses, or have been ignored.[2] A recent paper offers the first experimental attempt to relate what has been called an image to a definite muscular movement. Rexroad concluded that the visual after-image is either merely correlated with the tensions of the recti muscles, or is identical with these tensions.[3] Later, in his textbook, he teaches that the kinaesthetic stimuli arising from the muscle tensions when a visual stimulus is fixated, become in the absence of the visual stimulus the substitute stimuli for the verbal response 'I see an after-image.' He reasons that if the stimulus for the verbal report in his experiment is the pattern of muscular tensions and if one holds that the verbal report describes an after-image, "then *the after-image is the pattern of muscular tensions.*"[4] The supporting experimental findings show that if a subject is requested to fixate any point slightly to one side of a brightly illumined square, the subject reports with the cessation of the stimulus that the after-image seems to move in the direction of the stimulus square; and the experimenter may observe that the eyes are turning in that direction. Thus for each pattern of stimulation there is a resulting pattern of tensions in the eye muscles. It was further reported that in all cases where the initial fixation was to one side of the bright square the subject reported an after image only when the correlated eye-movement was present. If stimulation resulted in a tendency to move to the right, (fixation to left of bright square) the subject would report the absence of the after-image when instructed to follow a knot moved slowly to the left; but when the knot was moved to the right, the image was reported present. Rexroad further suggested that the reason no one had previously discovered that after-images were simply responses conditioned to the kinaesthetic pattern in the eye muscles was that with the usual conditions for obtaining after-images (direct fixation of the light source) eye-movements would not be obtained; the tensions in the recti muscles would be increased, but they would be balanced.[5]

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[1] Cf. J. B. Watson, *Psychology*, 1919, 48-112; *Behaviorism*, 1924, 51-60; W. S. Hunter, *Human Behavior*, 1928, 220-225; and A. P. Weiss, *A Theoretical Basis of Human Behavior*, 1925, 225-305. See also R. H. Gundlach, Some difficulties with Weiss' behavioristic postulates, this Journal, 38, 1927, 469-474.

[2] Watson seems first to have suggested that the image may be nothing but implicit motor responses (*Behavior*, 1914). Weiss puts the view forward with greater assurance (*op. cit.*, 235 ff., 276 ff.).

[3] C. N. Rexroad, Eye-movements and visual after-images, this Journal, 40, 1928, 426-433.

[4] C. N. Rexroad, *General Psychology for College Students*, 1929, 292.

[5] Rexroad, *op. cit.*, 291.

The results which Rexroad reported were so startling that we thought it worth while repeating the experiment with a few modifications in the instructions. It seemed possible that his naïve observers did not distinguish what they were fixating from what they were attending. Our *Os* were advanced students or members of the instructional staff. The technique was practically identical with Rexroad's. The *Os* were instructed verbally to fixate the small square indicated by *E* when the light was turned on; to try to retain the same fixation when the light went out; to report the color and direction of the movement of the image, if any; and to report the final location on the background where the image disappeared. For practice effects 6 of the 10 *Os* made a second, and three a third set of reports.

Rexroad says that his *Os* were unable to keep their eyes from drifting, and that movement was always toward the stimulus square when an image was observed. We find, on the contrary, that out of the 40 fixations in the first set of observations, 3 fixations were steady and without drift; out of the 24 fixations on the second days practice (6 *Os*) 12 fixations were steady. With our *Os* it appears that a different pattern of tensions in the eye muscles may accompany the same after-images.

Out of 65 drifts occuring while the image was being held, only 39 moved toward the previous stimulus-square; 14 moved in the opposite direction, and 12 moved at right angles to the square. The image, then, may drift in any direction from the original stimulus-square without being lost. In fact 6 of the *Os* had definite tendencies toward movement in some one specific direction, regardless of the relation of the fixation-point to the stimulus-square.

The *Os* were asked, after the image had been formed, to fixate and to follow a knot on a string that was moved about. Every one of the 10 *Os* could place the image on the knot and follow it in any direction, when the string was moved slowly. These results also stand against Rexroad who reports that the image is present only when the knot is moved in a direction coinciding with that of the eye-movements produced by the stimulation.

There must finally be mentioned that qualitative differences in the after-image, and its spatial pattern, are dependent upon the nature of the preceding stimulation, and that it varies independently of any specific pattern of muscle tensions.

We find, contrary to Rexroad, no necessary relation between eye-movements and after-images, and conclude that the image is dependent, as most theories of vision hold, upon the chemical after-effects of retinal stimulation.

University of Washington

V. W. Walters
R. H. Gundlach

A Reply to Dr. Murray's Concept of Aesthetics

Dr. Murray, in the October, 1930, issue of this Journal,[1] criticized rather severely the concept of the "aesthetic" which she believed was implied in a study by White and Landis.[2] Dr. Murray presents an illuminating discussion of the difference between mere conventional prettiness and the true aesthetic character

[1] E. Murray, Some uses and misuses of the term 'aesthetic,' *op. cit.*, 42, 1930, 640-644.

[2] R. K. White and C. Landis, Perception of silhouettes, *ibid.*, 431-435.

of the work of such great original geniuses as Beethoven, Wagner, or Rodin. Our study, she asserts, since it deals only with what is pleasing or displeasing in the usual sense of those words, is irrelevant to the real problem, which is to discover why things are or are not genuinely aesthetic.

That this distinction is an important one, we heartily agree; but when Dr. Murray refers to our main conclusion as a "chance correlation," or when she denies that such work as ours has any right to recognition in the field of aesthetics, we must take issue with her. Our conclusion was that, with one type of material (the human profile) and with one type of observers (children and college students), there is a tendency for familiarity to produce pleasure and for unfamiliarity to produce displeasure or dislike. Three separate lines of evidence pointed to this conclusion. Although the critical-ratio technique could not be applied, we are convinced that chance alone could not give as great a difference more than once in a thousand similar experiments; and we believe that a careful reading of our paper will convince anyone that the possibility of "chance correlation" is so small as to be completely negligible.

We must take issue with her, too, about the place of such experimentation in a comprehensive science of aesthetics. Granted that Dr. Murray means by "the aesthetic" something on a far higher plane than what our subjects meant by 'pleasure' or 'liking' that does not mean that scientists interested in the former must shut their eyes to the latter. The reactions of approach and withdrawal, liking and disliking, are indeed simple, but for that very reason they can be experimentally studied at a time when the inevitable vagueness of Dr. Murray's more refined concept still makes it extremely difficult to approach. Within this broad field of aesthetics there is certainly some sort of genetic series from the most simple to the most complex and highly developed. At least the origin of the finest aesthetic feelings must involve simpler elements; and even in the highest forms of appreciation there must be some traces (scars, perhaps,) of their psychological forerunners.

Instead of narrowing our perspective, let us broaden it. Let us increase our knowledge, step by step, of the whole genetic series. Let us go down to the dance of the chimpanzee and up to the symphonies of Beethoven, with an attempt at each stage to discover under what circumstances a given kind of individual will react to a given kind of situation in a given kind of way. Where the aesthetic experience is quite ineffable, let us frankly admit the limitations of our analysis; but if it is in any way possible to obtain both trustworthy and specific data on any stage in the series, let these data be accepted and incorporated in the general scheme. This is a stupendous undertaking, but when it is finished we may have a body of knowledge that will throw fresh light even on Dr. Murray's concept, and give it a more vital meaning.

Stanford University — R. K. White
New York Psychiatric Institute — Carney Landis

A Rejoinder

In reply to White and Landis' criticism, let me point out that the offending phrase "chance correlation" occurs nowhere in my article. My actual words, "the chance discovery of a correlation between deviation and dislike," carry no

implications as to statistical error. They refer solely to the fact that these authors chanced upon this 'principle' while in pursuit of a quite different objective—the dependence of character judgments upon facial contours.

The *deviation-dislike* phenomenon (along with its correlative, *familiarity-pleasantness*) presents no novelty. Indeed, in any 'standard-variable' technique a pleasant toning may temporarily attach to the standard (tone, weight, or what not). In the silhouette experiment, *e.g.*, the initial use of another standard— a lean and frowning face rather than Lavater's sleek and smiling one—might conceivably have evoked as high an overlap with likability. Slight alteration of the instructions, the substitution of the method of paired comparison or of relative rank, would probably have reversed or nullified, for the actual profile used, the deviation-dislike relation reported. In this sense the correlation *is* chance, an artifact of the method.

My comment bore less, however, upon this particular datum than upon its relevancy (or the relevancy of the trend it disclosed) to the science of aesthetics.[1] In the socio-psychological field, preferences for the commonplace and conventional, the amiable rather than the intelligent, are items of no mean value, with reference to general theory. Physiognomist and phrenologist, moreover, have long made capital of 'optimal profiles' as cues to the best 'mixer' or the most desirable mate. But that the average of these 'optimal profiles' (inevitably as numerous as racial, cultural, age groups, as fads and fashions) should afford a universal standard of beauty, or a test of taste, is out of keeping with the principles of modern science, and the known diversity of human types.

The dispute turns in part upon the definition of 'aesthetic.' If the word is a mere synonym for the affective, the *familar-pleasant*, if a bath the temperature of which approximates the accustomed, a soup the seasoning of which conforms to the usual, is to be termed *aesthetic*, verbal economy and precision dictate that we drop the term from our psychological glossary. Usage, however, tends to assign the word a fairly definite rôle, identifying the aesthetic with a completer functioning of the individual than finds scope in our circumscribed practical perceptions; a peculiarly rich and integrated functioning exemplified as fully in the dance and song of the child or savage, as in the 'more refined' appreciations of the cultured adult.[2] This type of functioning, it is quite possible, may have less in common with the 'reactions of approach and withdrawal' than with the equally primitive one of *arrest*.

Now White and Landis, if I grasp their program, would restrict aesthetic investigation to the cataloguing of the agreeable, the listing of the familiar-pleasant; pivoting all aesthetic reactions upon their principle of deviation-dislike. This tremendous *a priori* simplification appears to me discredited in advance by the accessible facts. A cursory survey of Mayan, Hopi, Zulu dance and song and pictorial expression reveals the subordination of this principle to others more dynamic. Primitive art draws its aesthetic thrill from the startling and the sinister, more often than from the accustomed, the amiable and complaisant. Curiosity and fear play no less prominent rôles than the primitive satisfactions of

[1] Cf. the final speculative paragraphs of the White-Landis study, *op. cit.*, 435.

[2] Though favoring the extension of the term to a wide range of experience, simple and complex, I am inclined to balk at the extremes of White and Landis—the infra-human and the "ineffable."

sex and hunger. In the upper reaches of the 'genetic series,' critic and layman have voted beauty as often to emaciated or distorted contours as to the sleeker and more placid. Briefly, the aesthetic 'pleasantness' of a profile is a function of its congruence with the emotional purpose of the artist, rather than of the imagined social 'smoothness' of the original.

Investigation of the emotional expressiveness (as opposed to the mere 'likability') of profiles, tonal sequences, rhythms, is simple and feasible,[1] even with 'children and college students.' There are hopeful indications that the norms for expressiveness will be more compact and less chaotic than those for mere casual pleasantness, their assembling a less 'stupendous undertaking.' In any case, the search should jog us out of ruts, and enlarge the boundaries of art appreciation—and of the aesthetic.

Cornell University E. MURRAY

THIRTY-EIGHTH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

After a lapse of a year since the annual meeting of the American Psychological Association was merged with the Ninth International Congress of Psychology held at Yale in 1929, the thirty-eighth annual meeting was held at the University of Iowa, December 29 to 31, 1930. The books indicate a total registration of 382 persons, which is a good showing for a meeting place that was the farthest west in the history of the Association.

At the business meeting 15 new members and 187 associate members were elected. The following officers were elected: Professor Walter S. Hunter (Clark University) as president for the ensuing year; Professors Richard M. Elliott (University of Minnesota) and Clark L. Hull (Yale University) as members of the Council of Directors; Professors John E. Anderson (University of Minnesota) and Walter B. Pillsbury (University of Michigan) as representatives in the Division of Anthropology and Psychology of the National Research Council; Professor Mark A. May (Yale University) as representative on the Social Science Research Council; Professor Leonard Carmichael (Brown University) as treasurer for the term 1931-33; and Professors Roswell P. Angier (Yale University) and George F. Arps (Ohio State University) as representatives of the Association on the Council of the A. A. A. S. The Committee on the election of officers pointed out some inadequacies of the method of election, upon which the Association voted to have the names of all persons receiving more than ten votes on the nominating ballots placed upon the election ballot and that the vote shall henceforth be made and counted on the basis of a preferential and transferable balloting system. The Association further decided to canvass the membership in regard to holding the annual meeting in the second week of September or at the usual time in December. After a considerable amount of discussion it was voted to hold the meeting for the current year during the second week of September, the dates for which have since been set by the Executive Committee to be the 10th,

[1] A silhouette or shadow method modelled on that of White and Landis is one of the most promising suggestions in recent literature—though the use of two variables would furnish less equivocal data.

11th, and 12th of that month. The invitation of the University of Toronto to have the meeting take place there was accepted. A resolution was also passed urging the Council to secure and report on a mail ballot concerning the policy that in alternate years first consideration be given to invitations extended from institutions located in the territory west of the Appalachian Mountains.

The dedication of the new laboratory at the University of Iowa was celebrated in a special ceremony at which the following addresses were given centering on the general topic, "The Place of the Psychological Laboratory in Fundamental Relationships."

Systematic Psychology ..... Professor Howard C. Warren
Other Natural Sciences ..... Professor Walter R. Miles
Education ..... Professor Charles H. Judd
Psychiatry ..... Professor Edward A. Bott
Psychotechnology ..... Professor Robert S. Woodworth

The University of Iowa invited the members of the Association to a dedicatory dinner in the Memorial Union at which 350 guests were present. President Langfeld officiated as toastmaster at the speakers' table at which there were twelve ex-presidents of the Association and the President of the University. Following this there was a smoker in the ballroom of the Faculty Club during which Professor Langfeld read his presidential address on "A Response Interpretation of Consciousness." It developed a view of consciousness which reflected both the purely mental and motor aspects of the organism in its responsive attitude toward the environment. The paper was attentively followed and enthusiastically received.

There were 11 sessions in the formal program, every one containing 5 papers relating to various topics, such as general psychology, educational psychology, mental tests, child psychology, aesthetics, abnormal psychology, and college personnel. In addition there were two sessions each for experimental psychology and animal psychology. These programs ran in parallel sessions and drew an attendance of from 150 to 200 individuals for each group. Simultaneously there were also four round-table discussion groups on methodology and procedures concerning various special fields with the following numbers in attendance: on personality, 71; on the pre-school child, 75; on the behavior problems of children, 99; on emotions, 61. The section for graduate students listened to 10 papers on a variety of subjects, most of them in the experimental or statistical field. The annual meeting of the section of clinical psychologists was somewhat of a disappointment in that very few of the clinical psychologists who have been in attendance during previous meetings were present and the question as to whether the section should be continued could not therefore be fairly answered. There was an extensive exhibit of apparatus mainly contributed by C. H. Stoelting of Chicago, and an exhibit of books representing almost a dozen of the leading publishers. Both of these displays aroused a considerable amount of interest and proved to be very instructive. A special demonstration was also given by Professor Max F. Meyer on "Electrical *versus* Acoustical Hearing." By placing the electrodes from an amplified modulated current on the skin behind the pinna of the ear, Professor Meyer was able to reproduce tones thus heard.

Some of the round-table discussions proved to be unsatisfactory for lack of organization in advance of the meeting and because many who were not professionally trained in the respective topics participated in the discussion. This led in a

few instances to overcrowded conditions and considerable deviation from a central theme. Such discussions are intrinsically very valuable but can be made so effectively only through some planning in advance in order that comments from well-trained members can be elicited by the chairman. He should also feel free to exercise his authority as chairman to regulate the flow of discussion. There is a happy mean between the stiff formality of parliamentary procedure and the chaos of informal conversation. The writer believes that these round-tables should be given more serious attention and that the conference of experimental psychologists omitted during the present meetings should be reinstated.

The greatest amount of interest seemed to develop in the round-table on the behavior problems of children but it proved difficult to steer the group on any logical path. An attempt was made to define what "behavior problems" are in terms of their scientific *versus* their practical aspects. Scientifically the pattern of behavior was taken as a symbol of underlying psychopathological factors. The question also arose as to whether diagnosis should be made before there is adequate scientific knowledge. But there was considerable feeling in favor of continuing the diagnostic program with the view of increasing understanding. Grave misbehavior was contrasted with petty maladjustments and the problems of personality that do not lead to overt problems of behavior were also considered. Another topic well received was that of the pre-school child. The discussion centered around studies in both laboratory and non-laboratory situations. Cognizance was taken of the fact that the techniques for observation and analysis, even under non-laboratory conditions, have been considerably improved and that a more scientific attitude has developed in connection with the use of children as subjects under laboratory conditions. The discussion on personality, since the attendance at this round-table was unusually large, proved to be somewhat unwieldy. The main techniques that were discussed were those of case studies *versus* statistical analysis and the conflict waged principally around the problem of their respective reliability. The point was made that the factor of individual interpretation loomed large in the analysis of case histories. The round-table on the technique and methodology concerned with the emotions also portrayed an attitude of scientific improvement in the direction of producing *bona fide* emotions under laboratory conditions. If objective and scientific attitudes prevailed observers objected no more to an investigation of their emotions than to an examination of their intelligence. Detailed criticisms were passed concerning the perfection of the electrical techniques involved and the type of stimuli that could be used to produce certain emotional effects. There was also some discussion about conditioning emotional responses.

A strong tone of the experimental method prevailed throughout the sessions, with a minimum of theoretical papers and a rather scattered sprinkling of test procedures. Considerable emphasis was placed in several of the programs on physiological considerations in terms of the more modern tendency toward an integrating principle of neural connections.

An opportunity was afforded to inspect the new laboratory where a large number of experimental arrangements were shown covering the various projects under way on the psychology of music, the graphic arts, speech, reading, learning of motor skills, general acoustics, and emotion.

University of Iowa CHRISTIAN R. RUCKMICK

The Psychological Laboratory of Cornell University

During the past fall Cornell University has completed the remodeling and reconstruction of its psychological laboratory, located on the third and fourth floors of Morrill Hall—one of the three buildings first erected by the University.[1] The building, constructed of stone and wood, is very sturdy and is well adapted to the needs of the laboratory. The outside walls and the cross-partitions are of stone, varying from 24 to 30 in. in thickness; the floor and ceiling timbers are 4 x 16 in.

Both floors of the laboratory have been completely remodeled, as those previously acquainted with the laboratory can see by examining the accompanying floor-plans (Figs. 1 and 2). The lower floor (Fig. 1) contains the departmental offices, the elementary laboratory, and the lecture rooms for the laboratory and advanced courses. The upper floor (Fig. 2) contains the advanced and research laboratories, a seminary and reading room, an apparatus room, a store room, and the business and editorial offices and the stock room of this Journal.

Lower floor. The administrative office and the offices of the general instructors are grouped around the entrance hall, while the laboratory instructors have their offices in the heart of the laboratory. Twenty rooms, including two offices, a lecture room, a shop, five dark rooms (four small and one large),[2] and eleven daylight rooms (nine small and two large), constitute the elementary laboratory. Every one of the laboratory rooms is equipped with an apparatus case, a black board, a table, and several chairs, and contains apparatus and equipment necessary for a definite group of experiments. Visual experiments are performed in the dark rooms under the illumination of 'daylite' lamps, and the lamps, color mixers, campimeters, etc., are parts of the permanent equipment of those rooms. The students, who work in pairs, move from room to room as they complete a group of experiments. A hallway extends from the entrance hall to the lecture rooms for the advanced courses[3] and to the stairway leading to the upper floor.

Upper floor. The advanced laboratory, providing instruction in the psychophysical and statistical methods, consists of five rooms: a large central room, which serves as a lecture and computing room and is equipped with chairs and tables and with modern computing machines; and four smaller rooms, one a dark room.[4] As in the elementary laboratory, every one of the smaller rooms has its own apparatus case and equipment for the experiments assigned to it.

The research laboratory, also on the fourth floor, consists of a large central hall and fourteen small rooms opening off it (rooms A–N, Fig. 2). The central hall is used as a museum and for meetings and receptions. Every one of the small rooms has an outside window, and is supplied with D.C. and A.C. current and with twenty connections to the laboratory wiring system. Some of the rooms are

[1]For a description of the laboratory of 1900 and an inventory of the apparatus then available cf. E. B. Titchener, *The Psychological Laboratory of Cornell University*, 1900. The laboratory at that time was housed in eleven rooms on the top floor of Morrill Hall.

[2]These rooms are grayish, not black. Black rooms are, we believe, depressing and in most instances are unnecessary and undesirable.

[3]The lecture room for the elementary courses is located in another building. It seats over 200 students, and adjoins a well-equipped demonstrational laboratory.

[4]The ceiling and the upper part of the walls of this room are painted black; the lower part is painted a neutral gray, and may be covered when occasion demands by means of black curtains, thus providing one black dark-room.

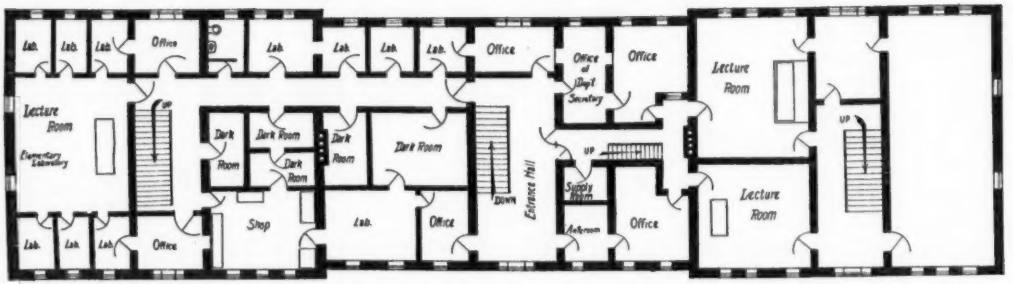

FIG. 1. LOWER FLOOR OF THE CORNELL LABORATORY OF PSYCHOLOGY

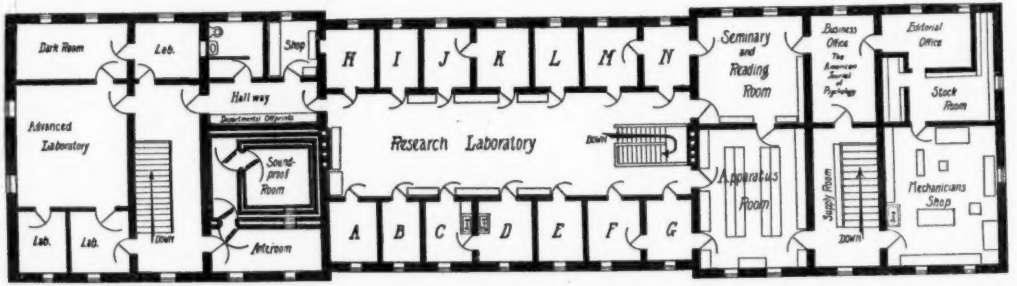

FIG. 2. UPPER FLOOR OF THE CORNELL LABORATORY OF PSYCHOLOGY

equipped with shutters, so that they may be converted into dark rooms; some are supplied with gas, compressed air and running water; and some may be used in suites (rooms C–D, F–G, J–K, and M–N). These rooms are assigned to the graduate students in accordance with the needs of their research.

Auxiliary to these research rooms are the apparatus, seminary, and soundproof rooms, and two shops, one for students and another for the Department's mechanician. The apparatus and the seminary rooms open off the large central hall and are thus conveniently situated for the use of the graduate students.

The apparatus room contains all of the apparatus and materials that are likely to be needed in research.[5] The apparatus is indexed and catalogued, and every piece has its appropriate place. It is checked out and in, and if it is out of order, is given immediately upon its return to the mechanician for reconditioning.

The seminaries and some of the advanced classes are held in the seminary room. Current numbers of psychological periodicals and some of the standard reference books are to be found here.

A shop, well equipped with the smaller tools, is provided for the use of the graduate students. Its location is convenient to the research rooms, but it is isolated from them by a heavy stone wall. The simpler work, incident to the construction and setting up of research apparatus, is done here by the student himself. The more complex work is done by the mechanician in his shop, which is equipped with the more intricate tools and with heavy machinery. This shop, as can be seen in Fig. 2, is completely isolated from the research rooms. The rooms immediately adjacent to the shop are store and stock rooms, and the rooms next to them are the apparatus and editorial rooms.

The soundproof room is also isolated from the rest of the laboratory.[6] It is, however, connected with every one of the individual research rooms and with an anteroom through the laboratory wiring system. It is also equipped with D.C. and A.C. current, compressed air, and a soundproof window. As can be seen in Figs. 2 and 4, the room lies between two heavy stone partitions, each 24 in. thick.

The construction of the room is shown in Figs. 3 and 4. Fig. 3 shows a cross-section in one vertical dimension, and Fig. 4 a cross-section in the other. The soundproof chamber is a room within a room. The construction of the inner room and of the end walls of the outer room is shown in Fig. 3. Passing from the outside and going in there is a layer of hard plaster, which is painted with a gloss paint so as to reflect sound. The plaster is spread on sheet rock. The supporting timbers are backed with insulite, and the interstices are filled with sawdust. Several layers of slater's paper are spread over the insulite; then there is a 2-in. air space, another layer of insulite, several layers of slater's paper, 14 in. of sawdust, and finally a third layer of insulite that forms the inner side of the outer room. The framework supporting this layer is completely isolated, by means of hair-felt,

[5]Until the remodeling of the laboratory, the research apparatus was classified according to use, and distributed among the rooms of the laboratory, then designated as visual, auditory, cutaneous, etc. This method of disposing of the apparatus is a heritage from Wundt. It was undoubtedly justified when psychological apparatus was simple and meager, but now it merely marks the persistence of a custom due to habit and inertia.

[6]The soundproof room was constructed with the aid of a grant from the Heckscher Foundation for the Advancement of Research, established by August Heckscher at Cornell University.

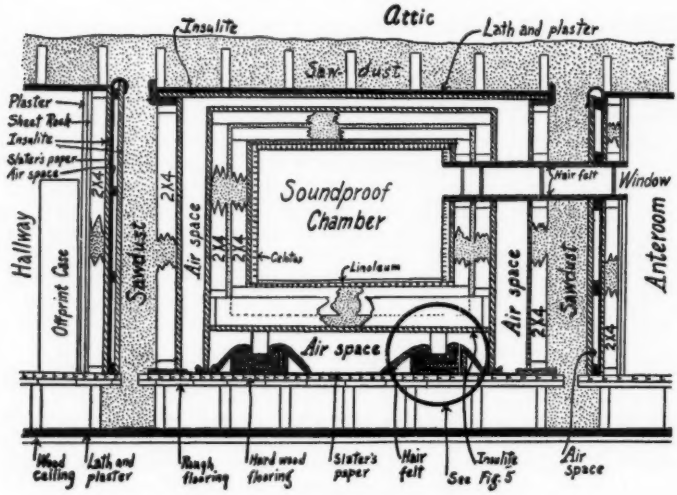

FIG. 3. CROSS-SECTION OF THE SOUNDPROOF ROOM
(East-West dimension)

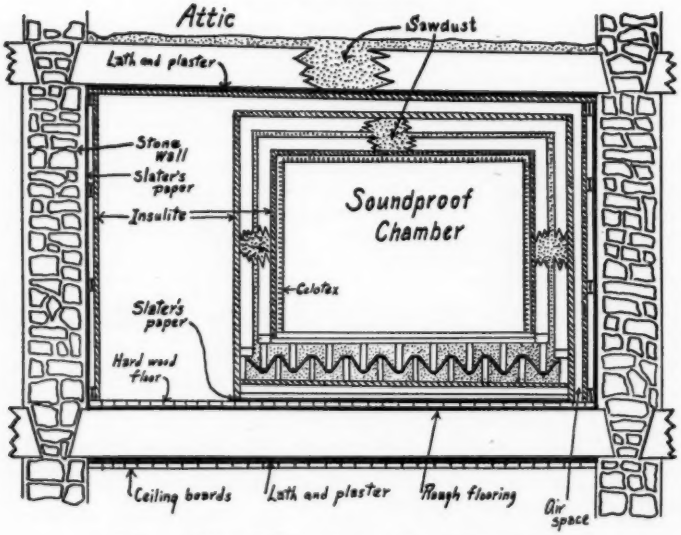

FIG. 4. CROSS-SECTION OF THE SOUNDPROOF ROOM
(North-South dimension)

from the ceiling and the floor; it is not even connected to them by nails. The partition is held in position by being nailed to the heavy layers of hair-felt, which in turn are nailed in place. The flooring and the ceiling between the two sides of this wall are broken, as shown in Fig. 3, so that they will not transmit vibrations to the inner room. The ceiling is isolated by means of a 16-in. layer of sawdust, lath and plaster, and a layer of insulite. The floor upon which the inner room is built is isolated from the room below by means of ceiling boards, lath and plaster, 16-in. joists and air spaces between them, rough flooring, hard wood tongue-and-groove flooring, and sheets of slater's paper. The construction of the side walls is shown in Fig. 4. Overlapping sheets of slater's paper are spread over the stone wall; then there is a 2-in. air space and a layer of insulite.

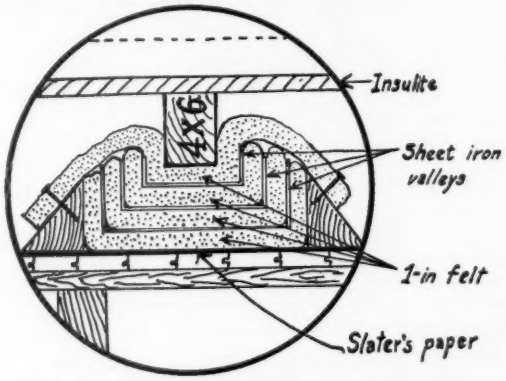

FIG. 5. DETAIL OF THE FLOOR-SUPPORTS OF THE SOUNDPROOF ROOM

The inner room is completely surrounded by an air-space. A 4-in. air-space is left between ceilings and, as shown in Fig. 2, between the walls on three sides. On the fourth side there is a 4-ft. passageway. The floor studding of the inner room rests upon five 4 x 6 in. joists—only two of which are shown in Fig. 3—which in turn rest in felt and metal gutters. The detail of these gutters is shown in Fig. 5. Four layers of one-inch hair-felt separated by sheet-iron valleys support the joists. The purpose of the felt and metal gutters—as of the entire floor construction—is to eliminate vibrations. Across the 4 x 6 in. joists is a layer of insulite, and over that is placed a second set of joists (2 x 6 in.) at right angles to the first. The outer wall and ceiling of the inner room are supported by these joists. They also support, by means of rope hammocks made of 1/2 in. hemp rope and held in position by staple irons, a third and parallel set of joists, which in turn support the floor and the inner walls and ceiling. The floor is made of tongue-and-groove hard wood. The space between the floor and the insulite sheeting nailed to the other side of the second set of joists is filled with sawdust. The outer and inner walls and ceiling are covered with insulite, and the spaces between them are also filled with sawdust. The room is to be lined with a sound absorbent and the floor covered with cork-linoleum and rugs.

The window of the room is made of four different thicknesses of plate glass. Every pane is set in felt, and is structurally isolated from the others. Every one rests upon a structural unit that has no connection other than a felt or sawdust one with the other units. The window greatly increases the usefulness of the room and does not noticeably decrease the soundproofing. When the window is not in use it is closed from both sides by felt and insulite shutters.

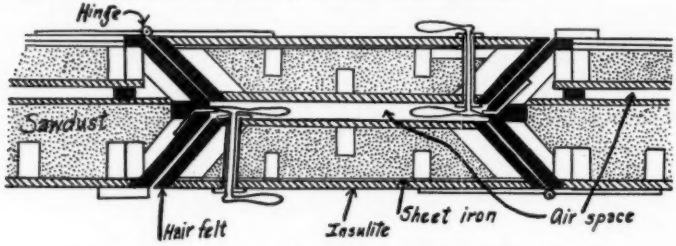

FIG. 6. DETAIL OF THE CONSTRUCTION OF THE DOORS OF THE SOUNDPROOF ROOM

The room is entered, as is shown in Fig. 2, through two double doors, the construction of which is shown in detail in Fig. 6. The doors are beveled, and the edges are covered with heavy hair-felt. The inner side of the doors is covered with insulite, and the outer side with sheet iron and insulite; they are filled with sawdust. The doors do not lock; they are held shut by levers, which draw the doors tightly into the beveled and felt-lined door-frames and may be operated from either side.

KARL M. DALLENBACH

THE SKIDMORE COLLEGE LABORATORY OF PSYCHOLOGY

During the forepart of the summer of 1930 Skidmore College acquired a large and valuable estate on which were located several substantial buildings. One of these was a two-story brick and stucco barn 50 x 114 ft.; another, close by, was a brick garage about 32 x 42 ft. with high walls and high pitched roof. The garage will be remodelled for work in comparative psychology; at present it houses the motor-generator for D.C. to supply the laboratories in the larger building.

The first floor of the larger building—formerly used as a stable for high bred horses—was made over for the secretarial department of the college. The second floor having apartments for servants and large storage spaces for grain and hay was alloted to psychology. This floor contains nearly 5400 sq. ft. The writer with the assistance of Dr. Hobbie, Superintendent of Grounds and Buildings, divided this floor space into 10 rooms as shown in the floor plan (Fig. 1).

The east-west location and the original form of the building directly favored the space arrangement and relative position of the rooms that would best serve the requirements of a psychological laboratory, *e.g.* the store room, lecture room, and soundproof rooms not requiring direct sunlight are located on the north side; and the elementary and advanced laboratories and dark room are placed on the south side, receiving full benefit of sunlight. The rooms and corridor are well lighted, doors are one-half glass except those to the dark room and soundproof rooms. The building is heated by a self-regulating oil burner and each room has

its own individual self-regulating hot water radiator; this feature makes it possible to hold every room at a constant temperature as required by the purposes of the room, *e.g.* the soundproof rooms require a higher temperature, owing to the use of tuning-forks, than store room or workroom. Hot and cold water is piped throughout and gas and A.C. and D.C. fixtures are in all desks as well as in the floors at suitable points in both elementary and advanced laboratories.

A switchboard located in the store room distributes D.C. in 10 different amounts of voltage ranging from 2 to 120 to 7 rooms; a seconds pendulum operating through the switch board controls time markers in these rooms.

The lecture room with 110 raised seats has a small platform at the rear with A.C. and D.C. connections for balopticon, slide projector, and movie reels. The

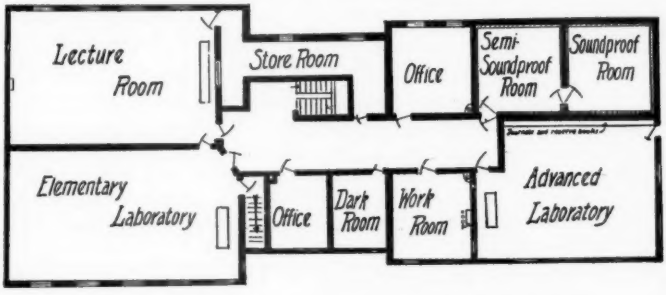

FIG. 1. FLOOR PLAN OF THE SKIDMORE PSYCHOLOGICAL LABORATORY

elementary laboratory, 40 x 24 ft., is provided with 15 heavy oak tables (will hold 18 comfortably when needed), 28 x 40 in., at which students are seated in working pairs.

The hood placed in the workroom is made of slate and thick glass thus permitting deodorization—a feature favoring olfactory experiments. The primary purposes of this room are statistical work, making simple repairs, mimeographing for the department, preparing mechanical drawings and smoking and fixing kymograph paper. Serious repairs and the construction of instruments are made in the college machine shop.

The semi-soundproof room is used for clinical purposes—examining ears, eyes, taking reaction-times, and timing reflexes and other motor responses. This room and the soundproof room are provided with heavy double doors closing against rubber facings. There are no windows to these rooms but the connecting doors when open give a distance of 30 ft. thus, making possible certain experiments in the dark which could not be carried out in either room separately. The effectiveness of the soundproof room against outside sounds and the apparent total absence of wall echoes may justify an account of its construction. A cross-section of the floor beginning with the ceiling of the room underneath and coming up would show: (1) soundproof plaster; (2) one half inch sheet celotex; (3) a 2-in. air space; (4) another layer of sheet celotex; (5) 1 in. of Blue-Stripe—a seaweed preparation for deadening sound; (6) a 2-in. air space; (7) and (8) two layers of sheet celotex

separated by a narrow air space; (9) heavy felt; (10) 1 in. flooring; and (11) 1 in. of cork preparation. A cross-section of the walls passing in from the outside would show: (1) stucco; (2) shiplap sheeting; (3) Blue-Stripe deadening; (4) air space; (5), (6), and (7) three layers of sheet celotex with small air spaces between; (8) soundproof plaster; and (9) celotex nailed to the plaster. A cross-section of the ceiling is similar to the walls except that tongue and groove pine flooring is used in the attic which corresponds to the stucco of the outer layer of the walls. The supporting floor timbers are 2 x 12 in. and the wall studding is 2 x 6 in. These two rooms are ventilated by electric fans drawing the air through detachable pipes.

The laboratory furnishings, the apparatus, and the methods used in teaching will, it is hoped, place psychology as an undergraduate subject on equal footing with other natural sciences.

Skidmore College LINUS W. KLINE

THE MOOSEHEART LABORATORY FOR CHILD RESEARCH

A laboratory of child research was established at the Child City of Mooseheart, Illinois, in September, 1930. All the children, about 1300 in number, are available as subjects. The children are approximately equally divided as to sex, and are normally distributed as to age—varying from infancy to the age of high school graduation. All the children are both physically and mentally normal. They

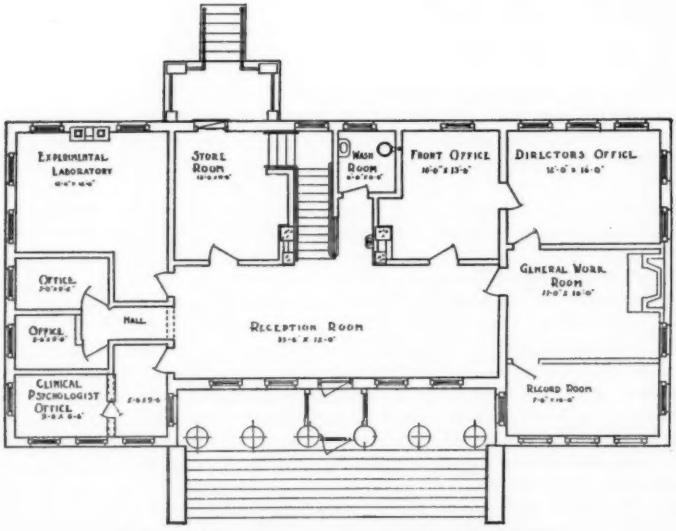

FIG. 1. MOOSEHEART LABORATORY (FIRST FLOOR)

are subject to 24-hour control and are available for study over a prolonged period of years. In these two respects at least, the new research center offers unique opportunities for child study.

The central unit of the laboratory of child research is housed in a separate building, the first two floors of which are shown in the accompanying diagrams (Figs. 1 and 2). The reception room, staff offices, and one laboratory and one workroom are located upon the first floor. The second floor, as the diagram shows, is devoted to laboratory and research rooms. Special rooms, located in the High School and

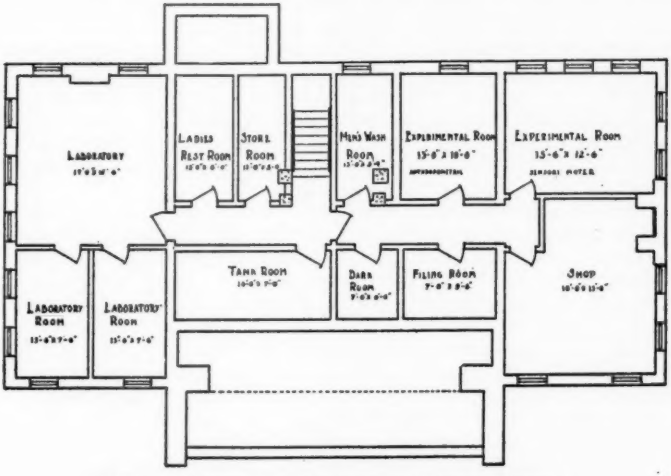

FIG. 2. MOOSEHEART LABORATORY (SECOND FLOOR)

the Baby Village, have also been assigned for child research. The facilities of the Children's Hospital, which is particularly well equipped, are also available.

An adequate appropriation has been made for personnel, apparatus, and library. Opportunities to use the facilities of the new laboratory will be accorded those interested and qualified. Fellowships, consisting chiefly of maintenance, will be offered to graduate students. Professors, on leave of absence from their own institutions, will be welcome to the use of the facilities of the institution.

Mooseheart, Illinois MARTIN L. REYMERT

HERMAN H. YOUNG: 1887–1931

Dr. Herman H. Young died suddenly of pneumonia at Bloomington, Indiana, on February 21, 1931, at the age of forty-three years. He was born in Uniontown, Ohio, on November 12, 1887. Dr. Young obtained his A.B. degree from Ohio University in 1913 and continued his graduate work at the University of Chicago and at Ohio University, obtaining his A.M. degree from the latter institution in 1914. This same year he came to the University of Pennsylvania to continue his graduate work and was granted the degree of Ph.D. at this University in 1916. As a graduate student, his interest was very largely in clinical psychology and his thesis was on the topic of the standardization of the Witmer Form Board.

During his graduate work, Dr. Young held several academic positions in psychology. He was assistant in psychology at Ohio University (1913-1914), instructor at the Indiana Reformatory (1915), psychologist for the Juvenile Courts at Cincinnati (1916), psychologist for the National Committee of Mental Hygiene (1917), Mental Examiner at the Children's Service Bureau at Youngstown, Ohio (1917-1921), and, during the last years of his service here, he was Director of the Bureau.

With this large background of practical clinical experience, Dr. Young then returned to academic work, becoming instructor in psychology at the University of Pennsylvania (1921-1922). In 1922 he accepted the appointment as Associate Professor of Psychology at Indiana University and in 1925 he was promoted to Professor of Psychology and Director of the Psychological Clinic at the same institution, positions which he held at the time of his death. He was a member of the American Psychological Association and since 1927 was chairman of the Section of Clinical Psychologists of this organization. He was married to Mary Hoover Young who also holds a doctorate in psychology.

Dr. Young is the author of some fifteen articles and monographs in the field of clinical psychology. His outstanding contributions are his standardization of the Witmer Form Board and of the Young Slot Mazes. He has been very active, during the last several years, in establishing the position of clinical psychology in the State of Indiana.

University of Pennsylvania SAMUEL W. FERNBERGER

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Das Gestaltproblem. By RUPPRECHT MATTHAEI. München, J. F. Bergmann, 1929. Pp. 103.

The purpose of this *Sonderausgabe* from *Ergebnisse der Physiologie* is to acquaint physiologists with the chief tenets of *Gestalt* psychology which, the author feels, throws new light upon basic biological problems. In presenting his subject the author has chosen a novel way of summarizing. What *Gestalten* and *Gestalttheorie* are is developed by a discussion of thirteen salient theses or principles which give in topical form the main facts of this school of psychology. Moreover, instead of presenting *Gestalt* psychology by way of an attack upon other systems of psychology, as is usually done, Matthaei takes what he believes the new movement has to offer and allows it to stand on its own merits. As a result, while much of the spiciness of *Gestalt* psychology is lacking, it will appeal to those who wish to know what this brand of psychology is like without the polemic that usually attends its exposition. That the author has succeeded admirably in handling the subject there can be no doubt. But since the principles discussed involve a consideration of fundamentals, often of disputed matters, this little book will be appreciated only by those who already have a fair knowledge of the subject. The discussion is concrete throughout and based upon experimental evidence, something which writers in English who vaporize about *Gestaltpsychologie* would do well to follow.

In this discussion of the problems of *Gestalt* more than in any other with which the reviewer is acquainted, the question of the relations of part to whole and whole to part is most frankly and sensibly attacked. At least six of the thirteen characterizations of *Gestalten* concern this matter of parts in a complex. While denying that parts and relations account for *Gestalten* and that analysis into elements is fruitful, this author admits that there may be various degrees of *Innigkeit* (inherence, coherence, togetherness) among the parts of a whole, and it is the degree to which the parts are bound to each other and the totality that determines the kind of *Gestalt*. In fact, while insisting in one of his chapters on the primacy of the whole, the author formulates several laws of *Gestalten* which (*O tempora! O mores!*) treat the *Gestalt* not as fundamental but as derivative from this relationship of parts. These laws refer to the variable rôle played by the parts in a whole, the inverse relationship between independence of parts and goodness of *Gestalt*, and the fact that structures are graded according to the extent to which the parts fit together.

But when the author attempts to derive one law of *Gestalt*, *Das Gestalt Gesetz*, we find him emerging with a number of laws which hardly seem at present to reduce to one. The law of *Gestalt* seems to be the law of *Prägnanz*, inner autonomy, completion, goodness, simplicity, or *Eigenleben*. Are all these reducible to one law? If so we are not clearly told which one, and how the reduction to one primary law is to be made. To the reviewer it seems as if the

primary law of *Gestalten* is that concerning the primacy of the whole, while the laws concerning the *behavior* of *Gestalten* are many, and not necessarily capable of reduction to one primary law.

Perhaps the most original part of this contribution, aside from the method of exposition already referred to, is the altogether too short last section wherein the author gives an *Ausblick auf ein psychologische Physiologie.* Matthaei finds present-day physiology predominantly analytic. Due to this fact important peculiarities of living organisms have escaped the notice of research workers. It is here that *Gestalt* psychology has something to offer to physiologists. Along the way pointed by *Gestaltpsychologie* physiology will begin again the study of the intact organism. Functions of single organs will then be seen as ordered within the activities of the organism as a whole. Here, too, we may look for a solution of the mind-body problem.

Some evidence of the growth of the literature on *Gestalt* psychology is gained from the bibliography which contains over 600 titles. Unfortunately, this bibliography has been largely copied, as the author admits, from the one given by Sander in 1927. It contains many references having little to do with *Gestalt* psychology directly and not a few of the errors of the earlier bibliography, showing that the author has not been to all the sources he mentions. Is there any justification for adding titles to a bibliography which one has not himself seen, even for the purpose of a bibliography? If there is, then at least the works referred to should be correctly listed. But apart from this matter the reviewer feels that Matthaei has done a good job in his presentation of a difficult subject.

Bryn Mawr College HARRY HELSON

Because I Stutter. By WENDELL JOHNSON. Introduction by Lee Edward Travis. New York D. Appleton & Co., 1930. Pp. xv, 127.

This is an 'inner' account by a stutterer himself, a graduate student in psychology at the University of Iowa, of his developmental history with especial reference to the difficulties imposed by this embarrassing limitation of self-expression. The account, though not wholly free from interpretations and theorizing, aims to portray what actually took place. The author emphasizes (what he regards as true at least in his own case) that the self-consciousness, the seclusion or withdrawal from social contacts, the compensation by means of athletic and other non-verbal competitions, by the cultivation of genial manners, and by the building of Utopias in which stuttering is banished and extreme self-expressions are manifest, are *results* of the stuttering which came on rather unheralded (and not sufficiently described, probably because of the lack of a clear memory of it) in about the fifth year. We are told that when he entered school at six he "studied and played eagerly. For about a year," he continues, "I had been stuttering slightly, but in school, as at home, nobody apparently paid much attention to my speech defect" (p. 13). He says that "a searching psychological analysis" revealed no "emotional disturbances" or "repressed complexes" at the root of the stuttering. The methods of the analysis are not described, but it is said to have been "carried on for about a year, and was terminated only when it had been shown to be, *by every psychoanalytic standard* ineffectual" (p. 8, the italics are mine).

Johnson denies that the stuttering was, as his father explained it, due to thinking ahead of his speech, because such stutterers "grow out of it." The mother's explanation that it was "a kind of nervousness" is also emphatically denied (pp. 8 f.). He holds, contrary to current views, that his shyness, emotional chaos, etc. did not *cause* but *resulted from* his stuttering (p. 20). He rightly, of course, maintains that dogmatism about explanations of the cause of stuttering in children leads to unsatisfactory treatment, neglect of getting expert attention, and even to positive cruelty from those who think children can voluntarily stop stuttering; but he seems to be too positive himself, later on in the account, in the view that his own stuttering, at least (and there is some tendency to generalize), was due solely to the fact, in his own words, "that I should have been left-handed. That means in simple terms that my native neural dominance lay in the right hemisphere of my brain. No one recalls that I was ever shifted from the use of the left hand to the use of the right; it is apparent that I learned to use the right hand because the large family with which I was surrounded was made up of right-handed individuals" (p. 93). This is the interpretation which the diagnostic laboratory tests "indicated," and it is also the one given rather categorically by Travis in the Introduction. There we read that the "neurological basis of stuttering. . . . . is a lack of a sufficiently dominant center of activity" (p. xiii), and also that the type of training given the subject, as well as other stutterers, is "based upon the concept that stuttering is a definite neuromuscular derangement of the functional type in which there is a general reduction of cortical activity . . . due to transient and mutually inhibitive activities of the associative areas of the right and the left cerebral hemispheres" (p. xii).

This view of inhibition by "rivalry between the two sides of the brain" leads in the Iowa laboratory, it seems, to "corrective treatment of stuttering the sole aim" of which "is to build up or establish a single dominant center of activity in the one or the other of the two cerebral hemispheres. This is accomplished in the main," says Travis, "by the development of a dominant motor lead through one side of the body" (p. xiv). Naturally left-handed persons who have been trained into right-handedness, as has our author, are brought back "to the use of the left hand in all major functions, especially writing" (*ibid.*). But there seem to be stutterers who are right-handed and who have never been forced to disturb this dominance; moreover, it seems that only "over half" of the group in the University of Iowa receiving the treatment indicated "have recovered completely or shown marked improvement" (p. xii), and 'controls' by other technique have not fully demonstrated the all-sufficiency of this method of treatment and interpretation. Indeed, to return to our author's account, it is found that, though he has greatly improved, he speaks with a slow deliberateness. "My speech," he says, however, "is improved by the practice of deliberate articulation only in its outward manifestation. It was only by getting at the basic neurology involved that my speech has been genuinely improved" (p. 121). It is not easy to say just how much of the improvement results from his coming, in the study of psychology, to look objectively at his own case and making it a matter of scientific study, or from his method of approaching the articulation of a sound by employing an easy vocalization, such as *ah* or *uh*, from which he glides "into the articulation

proper" (p. 114). By this method, combined with that of calming him down before he attempted to speak, the reviewer cured a six-year-old child completely of bad stuttering shortly after the period of incidence. For over a decade now there has not been the slightest speech difficulty. This child was right-handed, too! At any rate, it is urged here that dogmatism and overcertainty do not yet work well as to any interpretation or treatment of stuttering.

The reader of this interesting and instructive book is given a short but illuminating account of what went on in a "stuttering school" in which the author took treatment by the diligent use of "nature's method" for three months in 1923, and which he finally left with so much disgust that he has "hated that city ever since" (p. 84).

The stutterer is pictured by the author as largely excluded from social activity and hurt by and revengeful of a defect of his own which limits his self-expression and tends to set him off from others as inferior. (Hence his liking for other lines of competition, in which the victim in this case was rather successful.) He describes muscular tensions in the anticipation of meeting any embarrassing social situation, some of which anticipations "may precede the attempt to speak by a considerable length of time, even hours—in rare cases days" (p. 105); and he gives in the final chapter a more detailed account of specific speech reactions and difficulties, and methods of their alleviation or avoidance. Chapter III, on means of getting into the good graces of others by the cultivation of a pleasant disposition, should be widely read even by persons not specifically interested in stuttering! The entire book is valuable to all those interested in maladjustments and even to social psychology in general, for it is undeniable that many non-stutterers who have noticeable handicaps of one sort or another (certain racial characteristics, physical defects, etc.) which are not sheer feeble-mindedness, likewise build up self-consciousness, shyness, etc. and compensating Utopias, and develop methods of avoiding embarrassments. J. P.

Die psychologischen Anschauungen J. J. Rousseaus und ihr Zusammenhang mit der französischen und englischen Psychologie des XVI-XVIII Jahrhunderts. Ein Beitrag zur Geschichte der Psychologie. By WALTER FRÄSSDORF. *Päd. Mag.*, no. 1214. (*Philos u. psychol. Arb.*). Langensalza, Hermann Beyer & Sons, 1929. Pp. viii, 248.

This book represents a significant contribution to the history of psychology. It contains a complete and scholarly exposition of Rousseau's psychological theories, their sources, and their influence upon subsequent thought. Since Rousseau never gave systematic expression to his psychology, his basic doctrines are necessarily pieced together from several sources. But the author has handled his material so skilfully that the total gives the pleasing impression of unity and logical development. There is abundant evidence of careful planning and painstaking attention to details. References to sources are copious and precise.

One cannot read far in this volume without being impressed by the amazing modernism of many of Rousseau's psychological conceptions, or, to put it differently, by the extent of the debt of modern psychology to Rousseau.

Frässdorf believes that Rousseau made three major contributions of lasting value to psychology: (1) his conception of the mental life of the child as something distinctly and qualitatively different from that of the adult; (2) his insistence that mind is essentially active, dynamic, not merely a complex of automatic reactions to the environment; and (3) his emphasis upon the importance of feeling in the mental life, and his recognition of feelings as a distinct class of mental contents, thus antedating Kant. How modern it all sounds! Rousseau's 'naturalistic' rebellion against the materialistic sophistication of the French Enlightenment bears many striking points of similarity to the present revolt against purely mechanistic conceptions of mind, voiced in Husserl's phenomenology, the *Gestalttheorie* of the Berlin School, Krueger's *Entwicklungspsychologie*, and Spranger's *Geisteswissenschaft*.

Surely, with this excellent study on the market, no future writer of the history of psychology will have any excuse for omitting Rousseau from the psychological roster.

Bryn Mawr College E. FRANCES WELLS

The Psychology of Philosophers. By ALEXANDER HERZBERGER. Translated by E. P. F. Wareing. New York, Harcourt, Brace & Co., 1929. Pp. x, 228.

The title of this book is perhaps ambiguous. It might be taken to refer to the schools and tendencies of psychology favored by philosophers, *i.e.*, psychology and psychologists from the philosopher's point of view. Or it might be taken to mean a psychological analysis of the personalities of philosophers, *i.e.*, philosophy and philosophers from the psychologist's point of view. This ambiguity, however, is immediately dispelled when one turns to the book itself, for it deals with the psychology of philosophers in the second of these two meanings. In other words, it is a clinical, case-history study of thirty of the great philosophers, from Socrates, Plato, and Aristotle, to Spencer, Hartmann, and Nietzsche. As the author states the purpose of the book: "We may . . .ask what kind of person develops neuroses and what kind does not. . . . In the same way we may inquire how a person comes to develop his particular theory of life and what sort of person he must be to develop one" (p. vii). The view-point of the author is psychoanalytic in a very moderate and "expurgated" usage of the term.

The main thesis of the book is that strong inhibitions, preventing success in practical affairs, have led to a diversion of energies and activities into philosophical pursuits. "The unsuitableness of the philosophers for practical life arises from unusually intense inhibitions which greatly reduce the possible outlets of the practical impulses in action and thus force them into an alternative activity, that of philosophic thought" (p. 137). The predominance of idealism in the history of philosophy is explained in terms of compensation for thwarted practical interests. Men desire God and immortality "when they have been denied joy and satisfaction on earth" (p. 214). It is claimed that the materialists, who have been "in a small minority," have been at heart idealists, and "have felt the need of painting the reality of the physical world in as effective and striking colours as possible in order to drive out a tendency to idealism" (p. 217). The last chapter, on "the psycho-hygienic value of philoso-

phy," maintains the biological utility of philosophic thinking in the preservation of the individual. This hygienic value of philosophy, it is claimed, also holds for those who are not creative thinkers but are merely students and followers of the great philosophers. "The magic garden which a powerful spirit has created to shelter it from the inclemencies of life becomes a public pleasure-ground affording recreation and health to many of life's wounded who have no plot of earth of their own" (p. 222).

The book is well written; indeed it rises at times to real literary beauty, as the sentence quoted from page 222 shows. The conclusions are well supported by reference to biographical facts and by careful argument. In a field like this, in which experiment cannot be applied decisively, there will always be counter-arguments and different interpretations of the same biographical facts. The book is probably of greater value to philosophers than to psychologists, though it will be read with interest by both groups.

Syracuse University WESLEY RAYMOND WELLS

Outlines of the Psychology of Religion. By HORATIO W. DRESSER. New York, Thomas Y. Crowell Co., 1929. Pp. xiii, 451.

During the past twenty-five years there has been a marked development in the psychology of religion. The general tendency has been to follow a pioneer author or to emphasize a single type of psychology or viewpoint. In *Outlines of the Psychology of Religion* the author has attempted to correlate and analyze in an impartial manner the leading topics of interest to both theologians and psychologists in the field of religion, making use of material in philosophy as well as in psychology and religion. At crucial points where many contemporary writers are markedly negative and critical, the author's method has been constructive.

The chapters in the early part of the book are concerned with such subjects as worship, prayer, faith, and belief. These are followed by the more strictly psychological topics, such as conversion, mysticism, personality and the subconscious. The later chapters are more theological and philosophical dealing with such themes as elements of religion and rationalism. Throughout the book the discussion is replete with citations and quotations from other works.

In the final chapter the author states more definitely his own viewpoint on religion. Religion is a way of life, an idea that is not new since it was taught and lived by the great founders of religion. Jesus is an example. Consideration of the religious life from this viewpoint would reveal that religion as the path toward perfection "involves a method of striving toward what *ought to be* through wise adjustment to that which exists."

This book is a good example of erudition being detrimental. To a large extent, the treatise is composed of quotations and citations. For the entire book there are, on the average, approximately 1.3 citations per page definite enough to require footnote references. On the whole the book is highly conceptual mainly because citations of illustrations are scanty or entirely missing in most sections. It is profoundly regrettable that adequate examples similar to that in Chapter XXV (pp. 401-403) are not utilized in other parts of the treatise. There is really very little psychology in the book.

However, because of the numerous citations, this book will be useful to students of religion and psychology as a source of references to the rapidly growing literature in the field.

University of Minnesota MILES A. TINKER

The Value of Homogeneous Grouping. By T. LUTHER PURDOM. Baltimore, Warwick and York, 1929. Pp. 99.

At the beginning of the fall semester in 1923 about 700 students in five high schools were given the Terman Group Intelligence Test. Two equivalent sections were then secured in each school by pairing students on three points, namely, intelligence test scores, age, and sex. In one of these two sections (the experimental) the students were further divided, in terms of intelligence, into two or more sub-groups, the exact number of such depending on the size of the enrolment in each school. Similarly, the students in the other section (the control) in each school were divided into the same number of sub-groups, but the attempt was made to preserve heterogeneity by having all amounts of intelligence represented in each sub-group. Through this experimental set-up it was hoped that data might be secured on the effect of homogeneous grouping on school achievement.

Further details of the procedure are: The experiment was confined to the two subjects, English and first-year algebra, in which standardized tests of recognized worth were utilized to secure initial and final measures. In so far as possible all teachers who participated taught both experimental and control groups and did so without making any adaptations of instructional materials or methods for the peculiar demands of the different groups. And finally, the same amount of subject-matter was presented in the two types of section during the one semester in which the experiment was carried on.

The final comparisons are made in terms of standardized test scores and teachers' marks. The experimental data are fully presented in the report for each school, each section, each subject, and each pupil. Simple statistical treatment yields as the two principal conclusions the facts that the students in the homogeneous sections did not gain more than those in the heterogeneous sections, and that homogeneous grouping was equally ineffective for all levels of intelligence.

While the study seems to have been carefully done, the findings will be variously acceptable. The psychologist who still sees in intelligence an all-powerful faculty will be at some loss to account for the failure of the homogeneous groups to make the expected gains. However, the psychologist who recognizes the limitations of intelligence tests and who realizes that at best an intelligence test score represents only an average of performances of somewhat uneven abilities will not be surprised at the outcome of the investigations. The latter may well doubt whether true homogeneity with respect to English and algebra was actually obtained by grouping students on the basis of their intelligence test scores, and he may therefore question the investigator concerning his right to phrase his problem as he has.

The practical educationist is certain likewise to quarrel with the investigator. He will insist that the potential advantages of grouping homogeneously

with regard to intelligence were lost in this study through failure to organize the curriculum and methods of instruction in such a way as to make capital of this homogeneity. While this charge may be admitted, the Purdom study may well be justified if it serves to convince school administrators that there is nothing magical about intelligence and about intelligence test scores; the former cannot operate in a vacuum, and the latter are not most wisely used when they are accepted and employed blindly and uncritically.

Peabody College — W. A. BROWNELL

Study Manual in Elementary Statistics. By E. F. LINDQUIST and G. D. STODDARD. New York, Longmans, Green & Co., 1929. Pp. ix, 109.

This is a working manual for students in elementary statistics designed especially to develop skill in sound statistical judgment as well as in the mere calculation of statistical constants. There are twenty-three units in the manual, each designed to cover a special aspect of statistical work, such as median and mode, geometric mean, frequency polygons or histograms, variabilities, standard measures, correlation coefficients, etc. One unit is devoted to logarithms. The usual topics in a course of this nature are well covered up to and including an introduction to partial or multiple correlation. Each unit contains references to Garrett, Holzinger, Rugg, Mills, Thurstone, Odell, *et al.*, and not only material for statistical work but also a number of questions for discussions. These seem to be well chosen for the purpose in view. The manual is one of the best of its kind in existence. One is disappointed, however, not to find a treatment of the effect of unreliability of tests on the magnitude of variabilities and on the reliability of measures of differences between groups. More attention to certain inaccuracies in the prediction of scores would also have been valuable. It is a question whether the subject of partial and multiple correlations ought to be introduced at all if not carried farther than in the final unit of this book. These and other slight shortcomings can easily be made up for by a good instructor. J. P.

Salvaging Old Age. By LILLIEN J. MARTIN and CLARE DE GRUCHY. New York, The Macmillan Co., 1930. Pp. 173.

Dr. Martin, one of the authors of this book, was at the time of writing seventy-eight years of age. The writers dedicate it to gallant old men and women. It is not a technical treatise for psychologists. Although we are informed that two hundred old people were investigated, comparative data or measurements are not presented. Instead there are brief and interesting anecdotes about particular types of old people which have value for the layman desiring interesting reading upon this phase of human development. To them it will be as interesting as a novel.

Physical training, exercises in visual observation, training in the mechanics of reading followed by verbal memory tests, and the discussion of a philosophy of life suited to each individual, are some of the psychological methods suggested for salvaging the old. Throughout the book emphasis is placed upon the development of intellectual interests as a means of keeping in touch with a constantly changing environment. An appeal is made to young people to permit

the old to live untrammelled by devices which profit the young more than the old. The fundamental assumption underlying the propagandist attitude of the authors is that the old are as subject to change and growth as at any other developmental period, but more time is required to produce changes than in youth.

University of Minnesota CHARLES BIRD

An Introduction to Social Research. HOWARD W. ODUM and KATHERINE JOCHER. New York, Henry Holt & Co., 1929. Pp. xvi, 488.

This is one of the volumes in the American Social Science Series edited by the senior author. It is an outgrowth of a specific demand at the University of North Carolina for the training of workers in regional social research. The book is intended as an introductory text, not as a guide for advanced work in methods of social research. It attempts to present a sort of unified picture of the range and difficulties of social research problems and to contribute toward a "definite scientific attitude on the part of the great majority of younger teachers of social science, and something toward the interpretation and promotion of the whole social research program" (p. v). The authors hope to stimulate young men and women of ability to secure thorough training for work in a new field which seems to be important and full of promise. The comprehensive outline and the necessarily very general and superficial treatment of the several special aspects of the subject seem to the reviewer not to be particularly stimulating to the scientifically inclined.

In working out the various "types of approach" (8 chapters), "types of method" (6 chapters), and "types of procedure" (6 chapters) in social science work, the authors show a commendable openness—now fortunately increasing in social science quarters generally—to aid from methods and results available in the more quantitative sciences, including psychology. This attitude has been greatly stimulated of late by the Social Science Research Council. The psychologist is interested especially in the chapter dealing with the psychological approach, but also in several other 'approaches,' such as the biological, the anthropological, and in such methods as the case-method, the experimental, the statistical, and the "scientific-human;" but he discovers in these chapters a too superficial skimming and quoting of views, at the expense of a careful analysis of helpful, specific contributions. This seems to be due largely to a lack of first-hand knowledge of such contributions on the part of the authors. For information as to psychological contributions that may be helpful, they seem to depend too much upon certain writers on psychological aspects of sociology who are not psychologists at all and who evidently are themselves not acquainted with many of the most valuable contributions by psychologists. However, the stress on research, the recognition of a need of better methods, and the open-mindedness toward related sciences is noteworthy.

The copious references in footnotes and the Representative Bibliography of 38 pages will compensate for certain of the weaknesses mentioned, but they serve also to emphasize the fact that the general make-up of the book is not very suitable for an elementary course. It is probably not fact-giving (particularly when the 'facts' are generalizations or opinions) but direction as to scientific technique in specific investigations that tends to develop research workers. J. P.

Abstracts of Theses: Humanistic Series, Vol. VI. Chicago, University of Chicago Press, 1929. Pp. xi, 456.

This volume contains the abstracts of doctorate theses submitted to the Graduate Faculty at the University of Chicago during the period September, 1927, to June, 1928. According to the regulations adopted in this institution May, 1922, and January, 1923, the publication requirements of candidates for the Ph.D. degree could be met by the printing of an abstract of the thesis. This was only an alternative plan, however, to that of publishing the entire thesis. In February, 1924, however, a modification of the rule made it encumbent upon all candidates to publish an abstract whether or not other publication of the thesis is made. The several departments encourage the publication of theses in full as well as of the abstract. Other details do not interest us here.

The departments included in the Humanistic Series are Philosophy, Psychology, Education, Economics, Political Science, History, Sociology and Anthropology, Home Economics and Household Administration, the several language departments, both ancient and modern, and the departments included in the Graduate Divinity School, the Graduate School of Commerce and Administration, and the Graduate School of Social Science Administration. There are in this volume five abstracts of psychological theses, some of which have been published in full in psychological journals. These are: (1) The Comparative Study of Three Psychological Methods, by Kate Hevner. The three methods here compared are the method of paired comparisons, the order of merit method, and the method of equal-appearing intervals. (2) Disintegration Effects of Continuous Running and Removal of Food Incentive upon the Maze Habits of Albino Rats, by Winford Lee Sharp. (3) A Preliminary Study of Ocular Attitudes in Thinking of Special Relations, by Edward G. Story. (4) Reaction Time in Discriminatory Judgments, by Isabell King Wallace, and (5) The Work Curve in Memorizing and Its Relationship to Retention, by Goodrich Cook White.

These abstracts in general seem to the reviewer to be too formally stated and to occupy too much space with mere general statements which can be of little service other than to fulfil the requirements of printing. Less attention to classification and more to a condensation of actual content, methods, reliabilities, etc. would greatly increase their value in cases where full publication is not made. It is particularly important to emphasize the need of such content abstracts or summaries in view of present problems concerning the expense to students of complete publication of theses and the present overcrowding of psychological journals. No one can better effect such condensation of any thesis than the author, if the requirement is made. J. P.

A Study of Problem Boys and Their Brothers. By the Sub-Commission on Causes and Effects of Crime. Albany, J. B. Lyon Co., 1929. Pp. 408.

This is a report by the Sub-Commission on Causes and Effects of Crime (William Lewis Butcher, chairman, Jane M. Hoey, and Joseph McGinnies with Raymond Moley, research advisor, and Garry M. Shulman, research worker) to the Crime Commission of New York State. The investigation and the

writing up of the report was mainly done by the research worker. The report is devoted to a study of the "histories of problem boys and the non-problem brothers and presents data on the differences in innate capacity and in family and environmental experiences that led one group of brothers along roads of good behavior and the other group into the children's courts"(p. 5). While this is the third study of this sub-commission based on individual histories, it is the first one claiming to utilize a psychological as well as a sociological point of view and technique. This study follows the case method technique. Intelligence test and mechanical ability test (Stenquist's) data are utilized and studies of psycho-neurotic tendencies are made.

Inadequate consideration is given to the effect of environment on both of these classes of tests, and so many data are massed together without adequate summaries and guides, and without sufficient check by careful scientific methods developed in other researches, that the reader's questions regarding reliability can not receive adequate answers. Results obtained indicate that the environment within the same family has in the past been taken too much as similar for all members: it is pointed out that (presumably innate) combinations of different traits in mental and emotional make-ups in the same family group act selectively on social environments, determining different behavior tendencies of different individuals in apparently the same environment. The results show that the problem boys studied were duller than their 'normal' brothers, with median I.Q.s, respectively, of about 0.75 and 0.86; and that they were less stably organized and inferior also in school work, being about 2½ times as frequently retarded in school as were their brothers. But in mechanical ability the problem boys were superior, though no evidence is given that this difference is an innate one. Of course, this problem may well be raised seriously regarding the other differences of the reverse type. The median age of both groups was 15 years. The study shows the remarkable complexity of factors affecting conduct and the impossibility yet of anything like reliable predictions of behavior even when the general environment is fairly well known. Much individual attention and skillful control of personal influences are requisite to the normal development of certain individuals even when physical environment is somewhat tolerable. J. P.

A Statistical Study of the Individual Tests in Ages VIII and IX in the Stanford-Binet Scale. By J. E. W. WALLIN. Ment. Meas. Monog. No. 6. Baltimore, Williams & Wilkins Co., 1929. Pp. vii, 58.

This study is a critical and statistical analysis of the performances of 1382 children on the eight- and nine-year tests of the Stanford-Binet scale. A given test of the scale is somewhat arbitrarily considered standard for a certain age if 66.6 per cent of 'normal' children of that specific age pass it.

Of the six tests found at the 8-year level (the alternative tests are not considered) the ball-and-field test proves to be too difficult (only 54.4 per cent of normal 8-year-olds pass the test) and the comprehension (74.6 per cent) and definitions (83.3 per cent) are too easy. The results on the 9-year tests indicate that making change (74.6 per cent), giving rhymes (77.9 per cent), and sentence construction (86.7 per cent) are too easy.

The subjects are grouped into the three 'brightness' classifications of 'normal,' 'subnormal,' and 'feeble-minded.' All of the tests on the 8- and 9-year levels differentiate the three brightness groups. One of the most significant facts is a confirmation of Burt's findings that the difficulty rank of many of the individual tests varies greatly in the different brightness groups. Ranking the six tests in order of difficulty for each of the three brightness groups the reviewer finds correlations of 0.50, 0.14 and 0.54 between the rank orders of the normals and subnormals, normals and feeble-minded, and subnormals and feeble-minded, respectively, on the six 8-year tests. There are too few feeble-minded 9-year-olds to warrant comparisons, but the correlation between the rank orders of the normals and subnormals is 0.71.

A comparison of the percents of normals, subnormals, and feeble-minded *of the same Binet mental age* passing each test shows that the younger and brighter children do consistently better in the ball-and-field, comprehension, definitions, and weight tests. Increased life age 'exerts' a negative effect in these tests. The older and duller children do better in the counting-backward, vocabulary, making change, date, and the reverse digits tests. In these tests increased life, age and experience seem to exert a positive influence. It would seem that the tests in the first group, being less influenced by experience, are more valuable as measures of intelligence.

The author in many cases seems to be satisfied with mere qualitative statements and classifications where more quantitative data could have been given to advantage. But few data are given as to the personnel of the groups tested. The readers might have been informed as to how the subjects were secured, on what basis they were selected, etc. Some data are given in the case of 357 children with I.Q.s over 90. The fact that five of the tests are found to be decidedly too easy and only one too difficult suggests that the group may have been somewhat superior. The author's own data indicate the importance of this point for purposes of standardizations and group comparisons.

The statement is made that the 66.6 per cent passing standard was arbitrarily accepted. An examination of the percents passing the tests considered correctly placed indicates that in actual practice a range, with 66.6 as the approximate median was used. This range might have been stated. There is still disagreement among authorities as to whether or not this is the optimal percent to use. The brightness classifications are qualitative, clinical groupings which might have been defined more precisely. The discrepancies found in Terman's standardization suggests Burt's findings, that only four of his sixty-five tests were allotted to the same age-groups by all of twenty other investigators. A given mental age is certainly not as precise a thing as it is usually supposed to be.

The study as a whole is very suggestive. On the basis of several such studies a revision of the widely-used Stanford-Binet scale might be made, which would considerably increase its validity.

University of North Dakota C. W. TELFORD

Mitteilungen der Kriminalbiologischen Gesellschaft. Band II. ANON. Published by the Association of Criminal Biology of the Univ. of Graz, Ulr. Moser, 1929. Pp. 216.

The first section of this communication is devoted to papers and discussions of biological, psychological and personality types in relation to crime and the administration of penal programs. While a knowledge of 'types' is considered useful in the study of criminals certain cautions are emphasized. The concept of character types is often inexactly used but this perhaps could be partially remedied by the erection of more exact and well defined types. The present knowledge of criminal types can be put to little practical use by administrators of penal institutions.

The reports in the second and third parts of the proceedings cover various topics. The value of an anatomical investigation of the cadavers of criminals is established by the discovery of numerous atavisms which are not evident in the morphology of the body as a whole. Other papers deal with the psychopathology of the habitual criminal, the value of instruction in criminal biology, programs and laboratories for investigations in criminal biology at universities and in police departments, the relation of criminal biology to legal philosophy, and the nature of crime in contemporary Russia. A majority of the reports stress the practical application of results of research. The adequacy of the reports in this communication will be gratifying to the reader.

The Association of Criminal Biology is an admirable example of the coöperation of administrators and researchers in penal institutions and municipal clinics with those in universities.

University of Minnesota MILES A. TINKER

A Modern Theory of Ethics: A Study of the Relations of Ethics and Psychology. By W. OLAF STAPLEDON. New York, E. P. Dutton & Co., 1929. Pp. ix, 277.

This book treats of such topics as self-fulfilment, pleasure; the theories of Field, Moore, Hobhouse and others; teleology in ethics; tendency, capacity and conflict in biology and psychology; meaning and ground of good, ought, freedom of will, moral zeal and ecstacy. The reasoning, though clear and logical, is not easy to follow because of the fineness of the distinctions made, and the determination to go to the bottom and find the essential elements underlying the truths of all ethical theories.

From the standpoint of ethics, the most significant conclusion is that ideal good is the greatest possible fulfilment of all tendencies and capacities of the world as cognized by man, or by a future super-man or God. This opens the way to the building of a science of ethics largely on an objective basis.

From the psychological point of view, the principal contributions are to be found in his discussion of will and cognition. His view resembles James's theory of emotion in reversing the usual ideas. Objects in the environment stimulate and arouse tendencies in organisms to change such objects. In conscious acts the tendency to conation of these changes is cognized. The essential element in such conation is not to produce subjective feelings, as claimed by hedonists, but to carry out the cognized tendency to produce an objective

change. The greater the variety of such tendencies and the more completely the objects arouse them, the more complete are both objective and subjective fulfilments. The fulfilment is greatest when other persons, society in general, and the world as a whole, have capacities and tendencies fulfilled, not only separately, but emergently. Conation is theoretically determined and predictable, supposing complete knowledge of the world and the individual; yet the individual has many capacities and tendencies, any one of several of which he feels are realizable by integrative adjustments. But this may be an illusory feeling. There is a feeling of constraint to attain the largest of the possible fulfilments cognized, and this is the ought of ethics.

The finer points of these psychological and ethical theories cannot be adequately expressed in a review or brief abstract, but must be gained from a careful reading of most of the book itself. Those interested in the problems discussed will find it worth while to spend time in getting and considering the ideas set forth by the author.

Leominster, Massachusetts E. A. KIRKPATRICK

Diet and Efficiency: A Five-Year Controlled Experiment on Man. By HAROLD H. G. HOLCK. Preface by A. J. Carolson. Chicago, The University of Chicago Press, 1929. Pp. ix, 72, 12 charts.

This is a report of a detailed experiment by and on the author to test out the alleged effects of a (modified) "fletcherizing." The experiment extended (with one interruption by illness) from January 13, 1922, through April 10, 1928, and consists of a series of technical physical and mental measurements during a long pre-fletcherizing period (including a period of illness and of a marked restlessness), a fletcherizing period of 75 weeks, and a post-fletcherizing period of approximately 30 weeks. There were considerable intervals of intermission between these several continuous periods. The experiment was a careful attempt to test the principles of Horace Fletcher, who, when 44 years of age, was rejected as an insurance risk and who claims that "by waiting for true appetite before eating, by masticating and insalivating until the food 'swallowed itself,' rejecting what there was no desire to swallow, and ceasing when he craved no more, he put himself into an excellent mental and physical condition" (p. 6). Careful records were kept, by methods the details of which cannot here be described, of food and water in-take, with eating time; number of defecations; sleeping hours; urine (total phenols, uric acid, total nitrogen, total phosphorus, inorganic sulphates, and, for one year, total acidity); blood pressure, pulse rate, and oral temperature; basal metabolism, body weight and surface; and of salt intake, drugs used during a 98 day period of restlessness in part of the control period, as well as the occurrence of 'colds,' hunger, etc. These data are given in detail both in tables and graphs. During the experiment certain physical and mental efficiency tests were also taken regularly. These were: ergograph tests of strength, duration, onset of speed, etc.; typewriting tests; mental multiplication tests of two-cipher figures; and certain chess problem-solving tests. These tests are described in great detail and involve variations in details of method which we cannot attempt here to describe. They were, however, carefully controlled as to variable elements.

Results show that during the 75 weeks of fletcherizing the eating time per thousand calories was doubled. Food intake was lowest but nearly the same percentage of protein, carbohydrate, and fat was maintained in the food. Muscular endurance, accuracy in typewriting, and basal metabolism were diminished during fletcherizing. There was no significant effect on blood pressure, pulse rate, oral temperature, sleeping time, efficiency in mental multiplication and speed in typewriting. Efficiency in solving chess problems was increased, as well as body weight, during the fletcherizing period. While the experiments were carried out with commendable thoroughness, it is frankly recognized that the validity of results is somewhat limited by the fact that only one subject was used. The nature and duration of the experiment, make the inclusion of many subjects a practical impossibility. J. P.

Psychology for Life Insurance Underwriters. By W. T. Root. Chicago, A. W. Shaw Co., 1929. Pp. xiv, 250.

New books for life insurance underwriters are issued fairly regularly. The present volume is designed for classroom use, and also for the agent in service. It deals primarily with the field of general psychology. By eliminating the words "insurance" and "underwriter," and changing a few of the illustrations, it could be adapted to appeal to any unspecified group. It differs from most books in this field in that it does not contain "selling talks" and detailed reports of the agent's activities from the time he obtains a prospect's name until he has delivered a policy to him. It contains fifteen chapters dealing with such topics as the following, Personality of the Salesman; Instinct, Innate Tendencies and Emotions; Habit and Human Behavior; The Psychology of Suggestion; The Psychology of Advertising; Personality, Character and Individuality; etc.

Root introduces his discussion of the topic "How to Organize Your Thinking More Effectively" with the following statement: "There are four types of learning, viewed from the practical standpoint. They are (1) rote learning; (2) associative, logical, or rational learning; (3) cramming; (4) skill in using references" (p. 13). One wonders if even the psychologically untrained life insurance underwriter will fail to note the over-lapping in these classifications.

The author has prepared an extremely readable text which will probably have wide use. His reading lists are carefully selected. The student who follows these leads conscientiously will develop considerable background in the field of recent psychological literature. He may become intensely interested in psychology. Whether he will sell more life insurance as a result of this study is problematical.

As with most books of this type, one main fallacy is apparent. The material is organized into a fairly logical system, based upon current psychological doctrine. The ability to master the details of this system probably is highly correlated with general intelligence, as at present measured and defined. Nevertheless, we know from the extensive studies of the Bureau of Personnel Research at Carnegie Institute of Technology that a low correlation exists between general intelligence and commissions earned in selling life insurance. One would like to know the relationship between the ability to master the con-

tent of such texts and actual success on the job. We need objective proof that books of this type will increase the earning power of those who study them. Effective courses in psychology for life insurance underwriters may be written after the behavior of eminently successful underwriters has been studied. Some of these men are making a hundred thousand dollars a year. However, we venture to predict that the psychologist who becomes sufficiently familiar with their techniques to train others to go and do likewise, will not spend his time in writing books—he will feel an urgent need to put his newly acquired knowledge into practice.

Cornell University RICHARD S. UHRBROCK

Introduction à la psychologie collective. By CHARLES BLONDEL. Paris, Armand Colin, 1928. Pp. 211.

The author in this work is striving to establish systematic and working hypotheses for collective or group psychology, to define its boundaries and to allocate to it the fields of investigation or research which, in his estimation, are inadequately handled by other types of psychology. Though Blondel begins with the attitude that he is only attempting to feel his way into the subject and says that "le present livre n'est pas une psychologie collective, mais une introduction à la psychologie collective," he leaves the impression, by the end of the book, that he himself feels as though he has actually solved most of the problems involved. Despite this apparent assurance, from his own point of view, he appears quite open-minded at the end of the book, where he urges that his position be taken only as a working hypothesis instead of a dogma, and that although the opportunity for service is incontestable, if so considered, it should be modified, altered, or even abandoned in light of the results it achieves.

It is the contention of the author that the human individual is always and above all profoundly social, that mental phenomena always have a social import, that collective psychology is a fundamental branch of psychology and that most of the question relative to intellectual, affective and volitional experiences, commonly considered in general psychology, should be taken over in whole or in part by collective psychology. Instead of being only a simple complement or adjunct to psychology, collective psychology, so considered, becomes the center or kernel of all psychology.

Consideration is given to the positions of Comte, Durkheim, and Tarde, and particularly to the contrasting points of view of Durkheim and Tarde, the former affirming that society and its manifestations constitute the realities whereas the individual and his activities are only abstractions, while the latter, Tarde, champions the individual and holds society and its manifestations to be abstractions. Blondel takes issue with Tarde, especially, and holds that Tarde's concept of the individual as antedating society and preceding it in genetic order is perfectly useless. The study of perception should be approached from the point of view of the specific principles of general psychology and from the standpoints of individual and collective psychology. It is the latter, or collective psychology, however, which defines the limits or fields of investigation

for the first two methods. In the study of memory it is absolutely essential that collective psychology determine, first of all, that which has been borrowed successively or simultaneously from the different systems of group ideas or representations. Again, every affective state comes to us only under socialized form and the characteristics it gets from the species are always secondary or obscured by those it gets from society.

As with any discussion involving concepts this book is quite philosophical in nature. It is, in fact, dangerously close to definite ambiguity in many places, and the form of expression, which is rather highly involved, frequently serves to obscure further the principle under consideration. The classifications, from time to time, of certain phenomena or experiences as objective facts would seem to be open to dispute. It is true, however, that the author, though not always clear as to his meanings or definite as to the bases of his classifications, does not fail to make the reader appreciate the importance of society in the individual's activity and thought, a thing of which general and individual psychology at times fails to become cognizant.

University of Kentucky PAUL BOYNTON

The Opium Problem. CHAS. E. TERRY and MILDRED PELLENS. New York, The Committee on Drug Addictions, in collaboration with The Bureau of Social Hygiene, Inc., 1928. Pp. 928.

The book contains 928 pages of text, nine appendices and a copious bibliography. As stated in the preface, the book itself is a review of the material submitted to the committee on drug addiction. This committee sought answers to the following questions: (1) What is the extent of the chronic use of opium and its derivatives? (2) What causative factors are involved? (3) What is the etiology of chronic intoxication? (4) What is the nature of chronic opium intoxication? (5) How should this condition be treated? (6) How may the problem of chronic opium intoxication be solved?

The material dealt with bears upon one or another of these questions. The wide range of sources includes medical literature and reports of commissions and conventions. Questionnaires were sent out to medical heads of institutions, neurologists and psychiatrists, members of societies, authors of materia medica, and other medical works, health officers, and chiefs of police. Over 6000 items were examined, and the most significant of the results are reviewed in this book. Although much of the work is very valuable from the medical, legal, and sociological point of view, only a small part is of significance to the psychologist.

The book begins with a consideration of the extent of the problem, which is summed up in the statement, "Whatever their inaccuracies, these surveys and estimates indicate sufficiently clearly the existence of a major medico-social problem to make the denial of the existence of a general situation far more dangerous than its affirmation." The history of the use of this drug is given in some detail, and the authors conclude that the chief influencing factors in the development of this problem include (1) the early unrestricted use of opium to relieve pain, (2) ignorance of the dangers of its continued use, (3) discovery of its values other than therapeutic, (4) the influence of such writers as De

Quincy, (5) the invention of the hypodermic syringe, (6) the influence of wars, (7) the practice of opium smoking, (8) the influence of the patent medicine industry, (9) the discovery of heroin, (10) the illicit traffic, and (11) the general indifference of the public. These varying causes have led to a wide-spread use of this drug among a broad range of social groups. As a result, it is pointed out, a multitude of problems has arisen "which for their solution require extensive study from medical, psychological, sociological, and possibly other angles."

The etiology of the opium habit has been minutely studied: while there is obviously only one direct cause, namely, the continued taking of opium in some form over a sufficiently long period either for therapeutic or euphoric effect, "there are also one or more predisposing causes such as the influence of environment, of heredity, of psychopathological tendencies, and the like." In most cases some combination of these causes has brought about the condition of addiction. So far there has been little study made properly to classify the cases by causes; among the predisposing causes, the question of personality make-up is clearly in the psychologist's field, and has hardly been touched by investigators as yet.

From this point, a large amount of material dealing with the general nature of opium intoxication, the pathology of changes both somatic and psychic, has been carefully considered, but few definite conclusions are reached. This part of the book, as well as the chapters dealing with symptomatology and treatment, is largely medical. The need for extensive research along a number of lines is emphasized throughout.

The last part of the book is concerned with the problems of opium control and is (as written) not of psychological interest. Little or nothing is said about methods of combating the evil through reform of the addict by moral suasion, change of environment, and similar measures, although these are mentioned in much of the literature examined by the authors. The volume serves a very useful purpose in bringing together a large amount of material dealing with all aspects of the opium problem. If there had been a psychologist on the committee, perhaps he could have drawn further conclusions from the material submitted, and made constructive suggestions in the study.

Kipling Road,
Wellesley Hills, Mass. J. B. KNIGHT

Jewish Influence in Modern Thought. By A. A. ROBACK. Cambridge, Sci-Art Publishers, 1929. Pp. 506.

This book, a commemorative volume, published on the two-hundredth anniversary of Moses Mendelssohn and Gotthold Ephraim Lessing, two figures prominent in the cultural renaissance of the Jews and influential in removing racial prejudice, is composed of a series of essays, many of which have appeared previously as journal articles. The author begins with the avowed purpose, not merely to list facts concerning Jewish notables, but to incorporate a system of interpretation based upon psychological foundations, on the basis of which one may understand the significance of Jewish contributions.

When a Jew becomes eminent he is often classed as a great Frenchman, a great German, etc. This, although encouraged by many eminent Jews them-

selves, tends to lessen the distinction of Jews as a racial group. Genius among Jews is evident but it is not generally appreciated that many movements, with a dominant personality at the head, are furthered by Jewish support.

"There are three men today who intellectually rule not only Europe, but the whole civilized world." These Jews are Albert Einstein, Sigmund Freud and Henri Bergson. Few will deny that the first two are leaders in influence upon contemporary thought, but many will question the right of Bergson rather than John Dewey, Bertrand Russell, or perhaps some other philosopher or scientist to be named as the third leader.

Not a few have been impressed by the fact that nearly all the leaders in the psychoanalytic movement are Jewish. The author emphasizes this point and admits that there is a great deal of truth in the conclusion that psychoanalysis is a Jewish inspiration determined by the racial antecedents of its originator. By a careful analysis Roback indicates that the actual reasons for the fact that psychoanalysis is a Jewish movement may be found in the "peculiar make-up of the Jew, who is analytical in a psychological sense, and who is constantly reflecting on the 'why' and 'wherefore' of everything. . ." The movement gives expression to the hankering after purpose which has always characterized the Jewish race.

Gestalt psychology, another important contemporary movement, was founded by a Jew, Max Wertheimer; and all but one of the important leaders in this movement are of Jewish blood. The fact that Jews have remained singularly aloof from behaviorism is considered by the author a tribute to their good sense.

In some parts of the book the author partially fails in his intention to interpret rather than merely to present facts. At times the reader will receive the impression that the author is trying to prove a thesis. However, the few minor defects do not lessen the general value of the work as a whole. Dr. Roback has presented an important study, one which leaves the reader impressed with the many and great contributions of the Jewish race to the civilization of the world.

University of Minnesota MILES A. TINKER

Social Psychology. By BERNARD C. EWER. New York, The Macmillan Company, 1929. Pp. ix, 436.

This is a carefully labored, comprehensive treatment of the subject indicated in the title, written with amiable but uninspiring benignity for the consumption of such undergraduates as find it inconvenient to give any considerable attention to intellectual interests. A minimum of factual material is presented, though much of the argument might be considered as inductive generalization from facts more or less well established. An example picked at random will show the *ex cathedra* nature of the bulk of the volume: "Wherever two wills meet in opposition, it [the "instinctive impulse of domination"] leads to struggle and victory. Selfishly or unwisely exercised it is perhaps the most prevalent source of unhappiness in human experience. The domineering manner, the insistence upon 'having one's own way,' inevitably of course in conflict with persons of more or less similar disposition, is the cause of friction and rupture in all the close associations of life."

It is difficult to say anything really pertinent about material of this kind except that it is trite and dull and time-consuming. For the intelligence level at which such information is new and interesting there are the newspaper special article and the Haldeman-Julius publications; a collegian has a right to a scholarly treatment—has a right to know, for example, that on the subject cited there is a very respectable body of factual information the intelligent evaluation of which promises a modicum of insight and control. At the risk of painting the lily, another passage, also unselected, maybe cited: "A scamp or a villain may be personally fascinating. Deference, grace of speech, tasteful discrimination sometimes cover a multitude of moral defects. Even vice may assume such delicacy that it escapes merited condemnation." This is almost to invite ribaldry at the hand of some robust folk-critic unawed by the academic taboos; scientifically considered, it appears to be an attempt, in the manner of a political candidate, to gloss over ignorance of the relevant information—viz., the degree of correlation between conformity to ethical standards and social agreeableness.

The book is probably strongest in its table of contents; social learning, leadership, morals and manners, religion and philanthropy, are promising topics if treated realistically and with due regard to the body of existing knowledge on the one hand and the intelligence of the reader on the other. Unfortunately both of these entities are treated as if nearly non-existent.

Clark University RAYMOND R. WILLOUGHBY

The Significance of Personality. By RICHARD M. VAUGHAN. New York, The Macmillan Co., 1930. Pp. viii, 302.

The present interest in personality studies is the warrant for a review of this work. The name of the book, from a psychology standpoint, is a misnomer and the work itself is not a contribution to scientific literature. The author, we feel sure, did not so design it. To sustain his views he presents numerous selected citations both from philosophy, ancient and modern, and from contemporary studies in psychology. The theory of evolution is adhered to. A criticism of the various views entertained by present day psychologists is offered. The book is a distortion of science rather than a contribution to it.

Personality, the vaguest part of the work, seemingly is an aspect or an attribute of a divine being. The nearest approach to a definition may be indicated in the quotation: "As personality is a growth and is now conditioned by factors which are not necessarily always essential to it, we must free the idea of its anthropomorphisms in order to apply it intelligently to the Supreme Being. Possibly, we shall have reason to believe that the complete idea of personality is not in ourselves but that we are only sparks thrown out by the lapidary's wheel from the parental diamond—sparks but not diamond." The work frankly states that a "world view in which religion can live and breathe" is sought. The chapters bear such captions as "The Strife of Opinion," "The Wonder of Self Discovery," "The Humanity of God," "The Spirituality of Nature," etc. The work is essentially metaphysical with an attempt on the part of the author to put religion and religious values in the cloak of science.

Virginia Military Institute ROBERT L. BATES

Camping and Character: A Camp Experiment in Character Education. By H. S. Dimock and Chas. E. Hendry. Foreword by W. H. Kilpatrick. New York, Association Press, 1929. Pp. xxiii, 364.

This is not a book on psychology: it is the description of an attempt at a private summer camp for boys (Camp Ahmek on the shores of Canoe Lake, Algonquin Park, Ontario) "to utilize current principles and procedures of education, psychology, mental hygiene and sociology in the achievement of 'character' outcomes in the lives of boys." In view of the fact that a sum of approximately one million dollars is spent by parents in the United States and Canada annually on sending their children to camps, the authors have deemed it worth while to attempt a sort of quantitative estimate of possibilities. The methods are, of course, crude as yet and the results tentative; moreover, the book smells a little of propaganda methods and over enthusiasm, even though the authors have purposely tried to be objective. The work suffers somewhat in its evaluation of results from a too conscious "moralization" of education; and the evaluation of results, from adequate independent controls. The work is, however, a good beginning in a desirable direction.

J. P.

Instructional Tests in Physics. By Earl R. Glenn and Ellsworth S. Obourn.

Instructional Tests in Chemistry. By Earl R. Glenn and Louis E. Welton. Yonkers, World Book Co., 1930. Pp. iv, 58 and iv, 74.

Twenty-five tests in physics and thirty-six in chemistry containing from 25 to 30 items each have been constructed according to the modern multiple choice, completion, true and false methods, etc. In physics, "Tests 1 to 7 cover mechanics; tests 8 to 11, heat; tests 12 to 17, electricity; tests 18 and 19, sound; and tests 20 to 23, light." In chemistry, there are tests of the knowledge of oxygen, hydrogen, measurement of gases, compounds of sulfur, colloids, etc. In all these tests there are drawings of pieces of apparatus, and diagrams of other physical and chemical processes. The single test is sufficiently reliable for class averages but for individual measurement three or four tests are necessary to raise the reliability to 0.94 or 0.95. These tests are comprehensive and complete and measure in detail the knowledge of subject matter. It is possible, however, that learning based on such emphasis on itemized knowledge clutters up the learner's experience with disconnected facts rather than with facts which are integrated into large meaningful units.

University of North Carolina A. M. Jordan

The Physiology of Love. By George M. Katsaimos. Privately printed with an anonymous Foreword. Boston, 1929. Pp. 326.

This is a rather free discussion of sex problems intermixed throughout with an extremely crude hedonistic psychology and with numerous irrelevances of a general nature. The book is not a scientific treatise, and it shows no evidence of careful scientific research; it is (greatly attenuated) opinion, good and bad, based on the observations and experiences of a physican and expressed in the characteristic dogmatic tone. There is no index. J. P.

Studies in Service and Self-Control. By HUGH HARTSHORNE, MARK A. MAY, and JULIUS B. MALLER. New York, The Macmillan Company, 1929. Pp. xxiii, 559.

The authors of this book have departed from magic, quackery, fatalism, mysticism, and guesswork in the matter of character education. In effect almost every chapter constitutes a thrust against the old idea that character traits are inherited. Much of the data relative to "environment and service," "environment and self-control," and the "influence of home and school" indicate that character is to a very great extent a matter of specific education.

The procedure of the authors is very unusual. In applying science to a subject which has been treated philosophically for ages they devised their own instruments of measurement, and used them in testing a wide range of conduct. In most cases the results seem to be fairly reliable, except perhaps those obtained from the test of sociability. As yet we are not prepared to say that an individual who has had two or three deep friendships is not as sociably inclined as one who has had twice as many superficial ones. Furthermore, a foreign boy might have a good number of friends or companions in his own group, but almost none among the native children. On the whole the devices used for testing are rather ingenious, and furnish a long start in the right direction. The application of psychology to the analysis and measurement of a vast number of non-physical, and non-intellectual traits broadens its scope and offers it a new, difficult, but important challenge.

Teachers College, Syracuse University ROBERT P. CARROLL

Psychologie. By HAROLD K. SCHJELDERUP. Translated from the Swedish by M. Leixner v. Grünberg. Berlin, Walter de Gruyter & Co., 1928. Pp. xi, 330.

The author of this general text-book is professor of psychology and director of the psychological institute, University of Oslo, Norway. In the present volume he has attempted, as we often do in America, to make a somewhat unified presentation of the empirically established (and, as always, some *un*established!) contributions of psychology in its various fields. Schjelderup has tried to steer clear of the old interminable philosophical speculation on the nature of the 'soul' and has confined himself to the results of modern empirical investigations. He has drawn more heavily than is usual in European books on the contributions of American psychology, both of the classical experimental and of the behavioristic varieties. Even the 'contributions' of psychoanalysis have been included. All these various data he has attempted to weave into a whole from a somewhat consistent point of view, tentative enough to incorporate the constantly increasing contributions of the growing science.

Beginning with the general problems, methods, and a brief historical outline of psychology, the author indicates briefly the innately determined equipment of man—receptors, effectors (muscles and glands), and the mediating nervous mechanisms. The receptors, including the proprioceptors, are then taken up individually in connection with their several stimuli. The data given are sometimes not well up to date. For example, the lengths of the basilar membrane fibers are taken from Helmholtz, despite the later work of Gray and others. The proprioceptive system, the basis of space-time perception, phenomena of adaptation, Weber's

law, specific energy of the receptors, etc., are considered under the head of the senses, and all these topics receive rather more space and more matter of fact treatment than they are usually given in introductory texts in America. They are doubtless, as a rule, to be read by a more highly selected group of students than one writes for in America. Perception of relations as the original basis of thought (going back to Von Ehrenfels' 'Gestaltqualitäten' in 1890 and including brief mention of the work of the *Gestalt* school) receive very brief comment. Then comes a considerable treatment of the instincts, based to a large extent on Galton, McDougall, Thorndike, and Cannon, but also involving contributions by Freud and by Watson on conditioned fear reactions. The original basis of character and intelligence come next. Inner secretions form the main basis of character and receive a rather good consideration in their various aspects even to the demonstration by photographs of results of treatments of human cretins and of experiments on animals dealing with changes in sex characters, in rejuvenation, etc. Under the head of intelligence, brain convolutions, intelligence determiners in the Mendelian sense (from Goddard), and influence of environment receive consideration. While the author holds to the importance of innate factors, not well understood yet, he admits that we know almost nothing as to the real basis of different personality traits and abnormalities, and the extent to which they may be susceptible to modification by environment. The treatment of attention, the basic principles of learning and of the higher mental development of man, which draws heavily on experiments by American psychologists, ends Part I on the Original Nature of Man.

Part II on The Mnemic Functions deals first with such general processes as imagination, association, synaesthesia, forgetting, engrams and unconscious processes, and free association reactions, and next with such more specific factors as are involved in perception of the third dimension, in illusion and hallucination, speech, thought and intelligence, recalling and phantasy. Intelligence testing, the method of determining I Q, and their distribution are briefly treated; also the significance of mnemic factors in the feeling life, particularly in the Jungean complexes, come in for consideration as well as various partially acquired and even voluntary activities. Voluntary activity is characterized by its involvement of self-awareness and self-feeling. "A thorough understanding of volition is therefore made possible only by an analysis of the I-consciousness and the development of personality" (p. 222).

The way is thus (though by an inversion) opened up for a treatment of Personality and Its Conflicts, which constitutes Part III. Here one gets a pretty direct dose of psychoanalysis, which the author apparently regards as the only method of entering effectively into the individual's make-up and ideals. Contrary to the trend in American psychology, the author, like many other European psychologists, is ready to trust the somewhat questionable methods developed by Freud and his more or less direct followers. Schjelderup defends psychoanalysis against the charge that the analyst finds what he seeks. His reply is the usual one which first charges the accuser with ignorance of the method and then asserts (without evidence) that "for the person who actually knows the psychoanalytical method there can scarcely be any doubt that it leads to a deeper understanding of personality as well as of character development than

any other single one of the known psychological methods" (p. 236). In this part of the book little note is taken of the more scientific contributions by certain American and other psychologists, small as these contributions may yet be. The 49 pages given to psychoanalytical methods constitute a rather large part for an introductory text on psychology, especially when we consider the fact that these methods have not yet been validated.

Group psychology, dreams, hypnotism, 'possessions' and automatic writing, mental diseases, and mysticism are also considered in Part III, which, in the main, is theoretical or at least non-experimental. Part IV, which covers only 19 pages, is devoted to applications to medicine, education, law, business, and industry. The last four of these treatments draw very largely on American data, and the first on psychoanalysis. Stress is placed on the general principles according to which original dispositions are being shaped into the 'self.' The applications to education are drawn largely from Watson's preliminary experiments on the conditioning of children's emotional reactions and Terman's work on measuring innate differences in school children, and this discussion duplicates in part earlier treatments in the book. There is, of course, no contradiction in the author's emphasis both on innate possibilities and on the modifications effected in learning processes and 'conditioning'.

Viewing the book as a whole, one feels that the first part is probably too sensory for those beginning the study of psychology. Mental life, moreover, does not begin with mere receptive processes. Response is very prominent from the start, much of it being initiated from interoceptive stimuli, due to metabolic changes which go on continuously. The organism thus begins as an active organization, and thereby to a considerable extent determines its own stimuli, a point which should be more fully stressed; and perceptual processes result gradually from this activity. They are not causes of the activity but only an aspect of it. There are, of course, always exteroceptive stimuli playing on the growing mechanism and affecting its behavior, and proprioceptive impulses early play their part, too. The arrangement of Part I is probably more logical (beginning from elements) than psychological. The theoretical parts of the book, including psychoanalysis, are not satisfactorily coördinated with the more definitely experimental part. One regrets that these parts are not fully up to the scientific standard set in the early part of the book. J. P.

Anales del Instituto de Neurologia. Edited by A. RICALDONI. Montevideo, Imprenta Nacional, 1928. Pp. 514, 283 figures, 12 plates.

A collection of forty papers on timely neurological subjects including a considerable amount of original work. They are well written and illustrated, reflecting sound judgment and a scholarly grasp of each topic.

A. Ricaldoni. (1) Classification of nervous diseases. (2) Relation of blindness to various organic diseases of the nervous system. (3) Intracranial hypertension with discussion of recent concepts of cerebrospinal fluid circulation. (4) Hyperalgesic reflexes in Friedreich's disease. (5) The "ratchet" phenomenon exhibited by muscles in paralysis agitans. (6) Syndrome of disease of the lateral wall of the cavernous sinus, probably of syphilitic origin. The author describes a peculiar symptom in which the sound eye does not converge with the eye on the

affected side due to the loss of convergent association and due to an attempt at functional compensation to avoid diplopia. (7) Post-traumatic Jacksonian epilepsy rapidly cured by fixation abscess in the muscles produced by injections of turpentine. (8) A movable platform for observation and graphic recording of the Romberg sign and other disturbance of station. (9) Asthenia produced by theobromine and its derivatives.

F. A. Caffera. Angioblastic cerebro-medullary sarcoma with special reference to the presence of angioblastic cells.

A. Isola. Vertigo and ocular cephalalgias. Asthenopias and headaches and also migraine are discussed.

F. Mérola. Surgical approach to the cerebral ventricles. Osteoplastic flap.

J. M. Estopé. Some fundamental concepts on disturbances of speech. A criticism of Marie's concepts of aphasia.

J. Cunha. (1) Cranial radiology. The cranial base and sella turcica. (2) Interpretation of radiograms of bones.

L. A. Surraco. Spina bifida as the cause of incontinence in young subjects. A discussion of cases, with radiograms of urinary tract.

H. J. Rossello. The physiology and therapy of parathyroids. Historical chronology. Symptoms of parathyroid diseases and extirpations. Relation to calcium metabolism and hormone function.

F. Ruvertoni. Radicotomy. Technique of section of sensory roots as indicated in neuralgias, tabetic crises and muscular hypertonicity. Ramisectomy of sympathetic rami and its uncertain relation to muscular tonus.

R. Talice. Pyrotherapy with treponema hispanicum in certain cases of nervous and mental diseases. The virus keeps well. The disease which it produces is benign, well tolerated and cures spontaneously without complication. The therapeutic results in general paresis and dementia praecox are not as good as those of the malaria therapy.

A. Gaminari. American trypanosomiasis or Chagas' disease. History, pathology, habitat of insect carriers, and complement fixation in diagnosis.

E. Claveaux. Neurotropic ectodermosis. Experiments with viruses of nervous diseases such as encephalitis lethargica, rabies and poliomyelitis, which produce cutaneous symptoms.

H. J. Rossello. Chronaxie. A history and definition, and explanation of the electrical excitability of nerves.

R. Talice. Clinical capillaroscopy and capillary pathology in various nervous diseases, illustrated with excellent plates.

A. Fontecilla. Intracranial tumors. Reports and résumé of some clinical cases.

A. de Castro. Cutaneous reflexes. Observations on the Babinski sign in anemic states; the palmar reflex and the defense reflexes.

C. Estable. (1) The histopathology of Friedreich's disease as it affects the cord and cerebellar cortex. Shows alteration and hypoplasia of Purkinje cells and degeneration of climbing fibers and basket cells. Some observations on the conduction paths in the oblongata. (2) Comparative studies on the cell structure of the retina of mammals.

J. M. Sosa. The Golgi apparatus in the Purkinje cells of the cerebellar cortex in experimentally produced avitaminosis and in starvation. The author finds a fragmentation of the intracellular Golgi net and functional relation between the nucleus and the net; also a hypertrophy of the Purkinje cells.

J. J. Estable. Innervation of the suprarenal capsule. A histological study of nerve endings, plexuses and nerve cells in the capsule.

A. Isola and J. M. Alonso. Pathogenesis of neurotrophic keratitis. Review of cases. Trigeminal paralysis does not seem to be the essential cause, but trophic changes are dependent on sympathetic innervation.

A. Ricaldoni and F. Calleriza. Unilateral paralysis of the last four cranial nerves (Collet-Sicard syndrome) resulting from an unusual postoperative meningeal metastasis from a thyroid adenoma.

H. J. Rosello. A study of action currents in muscles. The author concludes that the activity of the muscle is excited or inhibited by the nerve centers. Excited or inhibited muscle responds by the same mechanism. The inhibition is not a function of muscle but of the nerve centers.

I. M. DeAyala. Two cases of melancholia during the course of other neurological diseases.

A. Amargós. Myxedemopathy of heart with radiograms.

A. Ricaldoni and A. Isola. A congenital and familial disease characterized by adiposo-genital dystrophy, associated with retinitis pigmentosa and a polydactylia in a family of four children.

J. C. Plá. Treatment of nocturnal pollutions by injections of atropine sulphate.

J. C. Carrere and J. Cunha. (1) The duodeno-vesicular reflex and the evacuation of bile from the gall bladder, with radiograms. (2) Disturbances of the reflex.

J. Cunha. Cholecystography and the rhythm of Carrere, with radiograms.

A. Ricaldoni and J. C. Plá. The test of McClure and Aldrich during the incubation period of infectious states.

Cornell University JAMES W. PAPEZ

La renaissance religieuse. By P. ARCHAMBAULT, G. Bernoville, A. N. Bertrand, Le R. P. Yves de la Brière, L. Brunschvicg, F. Challaye, J. Chevalier, P. L. Couchoud, R. Fernandez, R. Gillouin, P. Masson-Oursel, F. Sartiaux, A. Spire, Le R. P. Joseph de Tonquedec, J. Weil. Introduction and Conclusion by Gerges Guy-Grand. Paris, Félix Alcan, 1928. Pp. 272.

This addition to the library of contemporary philosophy is of more importance to historians and philosophers than to psychologists. Whatever is of psychological interest in these essays written from diverse points of view is summed up in the initial and final remarks of M. Guy-Grand, who points out that there is indeed a religious renaissance in France, but that it is probably less permanent than many of the authors think. It is also assignable to perfectly definite causes such as war-fatigue, and is a general tendency not confined to any particular form of religion as some of the contributors hold. The first part of the work deals with the awakening of a new religious spirit in connection with historical religious movements, such as Catholicism, Protestantism, Judaism and

Humanism. The characteristic limitations of each group are revealed in the tone of the essays which ranges from the strict authoritarian or apologetic attitude of the orthodox sects to the latitudinarianism of the humanists.

In a second part of the book entitled "Two Aspects of the Religious Renaissance" the question of an appropriate philosophy of religion is discussed from both Christian and non-Christian viewpoints. Tonquedec, Chevalier and Sartiaux canvass the possibilities of a return to the philosophy of Thomas Aquinas or rather Neo-Scholasticism as a working philosophy for modern life. Tonquedec offers a rhetorical eulogy while Chevalier urges its acceptance as a philosophy of common sense, not too difficult of access to the average mind, strictly logical yet allowing for the demands of the heart. According to Chevalier, Thomism, especially in the 'progressive' form of Neo-Scholasticism, presents a judicious mingling of rationalism and empiricism, permitting the incorporation of new scientific knowledge as well as reasonable freedom in religious belief within the limits of theism. Sartiaux completely disagrees with the preceding, maintaining that Thomism offers neither a scientific method nor an organized body of principles. It is essentially a philosophical compromise in which the weaknesses are concealed by ambiguous terms or vagueness of thought.

Challaye believes that a new universal religion must arise and suggests that important elements of such a universal religion of the future may come from the Orient. The eminent Orientalist, Masson-Oursel, outlines the general conditions of a spiritual understanding between Orient and Occident. In our expectations of spiritual and metaphysical stimulation from the Orient we must not forget such facts as the mutual suspicion of the East and West, the modern decadence of the Oriental spirit through Western innovations, the profound difference in environmental conditions and cultural level, and the contrasting mental attitudes of the two halves of the world. Respecting the latter, Brunschvicg contributes an essay on the humanism of the Occident in which the mystical attitude of the East and the rationalism of the West are traced to radical differences in fundamental interests and values leading to scientific method and practical developments in one region and to metaphysics, mysticism, and quietism in the other.

In conclusion Guy-Grand notes the persistent disagreement among the contributors to the symposium. There remain irreconcilable differences, traceable perhaps to a fundamental distinction in types of religious consciousness. Our ultimate affirmations seem to depend upon temperament. The idea of Immanance, for example, will never satisfy the transcendentalists. The naturalists and supernaturalists are unable to find common ground. In fact, these great classes of thinkers cannot even accept the same data, define common terms of agreement, or follow identical methods of investigation, so that the hope of a really universal religion seem for the present chimerical.

University of Cincinnati CHARLES M. DISERENS

The Symbolic Process and Its Integration in Children: A Study in Social Psychology. By JOHN F. MARKEY. New York, Harcourt, Brace & Co., 1928. Pp. 192.

The author informs us in the preface that "this study was preliminary to a project in delayed reaction experiments with children during the period of language beginning," but says he was unable to find time to carry out the experi-

ments. It is regrettable that the larger part of the time employed in writing the book was not devoted to experimental work. A well planned experiment, even of limited scope, may be expected to supply at least a few solid stones to the edifice that science is laboriously building, whereas the value of a verbose theoretical discussion of a subject like this one is at best problematical. As it stands, the author's discussion is extremely diffuse and difficult to follow.

A symbol is defined as "an act or object which is marked off by behavior as a substitute for a stimulus-act or -object and a response-act or -object, and which is also at the same time set off by behavior as different from them." The author first devotes twenty pages to the treatment accorded by American sociologists to the origin and nature of the symbolic process, and shows the increasing importance attributed to social behavior in this connection. The remainder of the book is a defense of the "social behavior" theory of the origin and development of symbolic processes. One chapter is devoted to a discussion of the first words uttered by infants, one to the "use of personal symbols," and one to "personal pronominal symbols." These three chapters summarize well-known language data from child study literature, but in a way that brings out certain sociological values sometimes overlooked. The contention is that "symbols are acquired in social interaction," that "meaning exists [only] through action on our part," and that "with increasing age there is a decrease in obvious action content and an increase in refined action content." Symbolic development is at first centered around the self, but as symbolic integration proceeds self reference decreases. Accordingly, the symbolic process is "in essence a social process of personal and group interaction and interdependence." Then follows a brief chapter on the symbolic process and delayed reaction intended to emphasize the author's view that "the social-vocal behavior situation apparently furnishes the requisite mechanisms for true symbolic integration when associated with a sufficiently complex-behavior system."

Up to this point Markey has made some use of the data from language studies, even if only to hang a theory on them, but the remainder of the book is an entirely theoretical discussion of "the self and reflective behavior (thinking)," "symbolic origins and the group," and "the symbolic process and social control." The author holds that introspective psychology can contribute nothing to our understanding of the symbolic process. "To assume that so-called sense data and sensory processes are the elementary form of knowledge is not warranted. It is the social interchange in symbolic integration which gives us knowledge."

The eight-page bibliography which is appended is as diffuse as the text and none too discriminating. Titles of books and monographs are listed without name of publisher or number of pages.

Stanford University LEWIS M. TERMAN

Studien zur Sexualpsychologie und -pädagogik der Gegenwart. By WM. CLASSEN. Friedrich Mann's Pädagogisches Magazin, Heft 1212. Langensalza, Hermann Beyer und Söhne, (Beyer and Mann), 1928. Pp. 111.

Most works on sexual psychology or sexual pedagogy are either too elaborate or too elementary, so that Classen's condensed treatment of the most important problems in this field will be welcome to psychologists as well as to the general

reader. In the first section of the book various concepts of sexuality, both technical and popular, are discussed as an introduction to the evolution of sexuality, and to the analysis of sex differences. The author regards an adequate definition of the concept of sexuality as fundamental to further researches and draws freely on the writings of Freud, Stern, and Spranger in arriving at a definition. An exposition of the evolution of sexuality leads to a critical analysis of the problem of sex differences with special emphasis on methods of investigation. Meumann's principles of method are largely quoted with examples of actual application. In a further section, the tentative or possible results of a thorough investigation by other methods, such as the "personalistic" method of Stern, are discussed.

In the second part of his work, the author considers the nature and limits of sexual psychological prophylactics, and its justification in the light of psychoanalytical theory. Another section deals with the nature and value of explanations of sex phenomenon in connection with the education of youth. A particularly illuminating chapter is devoted to the development of the feeling of shame and its pedagogical significance. Methods are suggested of meeting the moral dangers which young people encounter during adolescence. Dr. Classen's work may be recommended as a concise yet thorough introduction to the principal problems of sexual pedagogy. It moreover affords an excellent basis for further investigations, on the part of teachers, physicians, jurists and others interested in this field. A bibliography of sixty-one titles is appended.

University of Cincinnati CHARLES M. DISERENS

Victim and Victor. By JOHN RATHBONE OLIVER. New York, Macmillan Co., 1928. Pp. x, 435.

This story of the emotional upheavals of an unfrocked priest (in the Episcopal church) is a study in psychopathology and the clinic that also throws rich incidental light upon the modern religious quasi-mysticism. It appears from the Preface that the book aims to show through the combined work of two men, a clergyman and a psychiatrist, how the difficulties of psychasthenic patients might be more effectively handled by a combined priest-physician than by a man of either profession alone. Several cases are described as under their care, though evidently more under the clergyman's than under the psychiatrist's; and the cases become unimportant on their own account, the main figure being that of the clergyman-patient himself.

If the objective be to show how religion may come to the aid of psychiatry, the goal is not well attained. The principal case dealt with is one whose trials result from fixation upon a highly specific religious objective and adjustment comes only as he finally satisfies the frustrated desire: his religion is as much cause as cure. The minor cases appear to become straightened out more by the milk of human kindness than by inculcation of religious practices or beliefs.

The matter-of-fact reader may find the style on many pages somewhat sentimental; yet it is well calculated to give him the surcharged emotional atmosphere in which a psychasthenic lives. He will also gain insight into the emotional authoritarianism that is essential to the attitude of a priesthood.

University of North Carolina J. F. DASHIELL

L'apprentissage du mouvement et l'automatisme. Études de psychologie publiées sous la direction de Albert Michotte, Vol. III. By J. VAN DER VELDT. Louvain and Paris, 1928. Pp. xii, 350.

The present thesis is a record of experiments conducted at the University of Louvain from 1924 to 1927. Twenty-seven subjects seem to have participated. The first section offers a résumé of work of predecessors and describes the purpose of the present work and the technical procedures employed in it. The second and third parts are devoted to the results which are further summarized in a section of general conclusions. An ample bibliography of eight pages facilitates criticism of the author's own experiments. The latter are concerned with the acquisition of various types of movement, simple and complex, spontaneous and voluntary, new and partially learned. The author studies separately (1) the execution and (2) the release of movements as distinct problems. His method is behavioristic in a broad sense of the word, *i.e.*, he examines both the external and internal conduct of individuals, recording both objective and subjective data. He distinguishes between the sensorial and real forms of movement and notes that in the course of learning the former gradually yields to the latter and the movement becomes automatic. The mechanism of the release of movement varies with the degree of automatism. There are differences between the results of sense training and those of sensory-motor training, such differences appearing in the subsequent effects of training as well as in the character of automatization, for example, the effects of sensory-motor training exhibit superior retention and are more precise. Differences are greatest during the middle of the training period. Sensory-motor training also yields a higher degree of automatism. A brief review fails to do justice to this very thorough piece of work, which is, moreover, difficult to summarize. The variety of tasks imposed, the distinctions of problems affected, and the voluminous introspections of subjects included, make reference to the original work almost essential to an understanding of the author's contribution.

University of Cincinnati CHARLES M. DISERENS

Introducción a la Psicologia Experimental. By P. M. BARBADO. Madrid, Editorial Voluntad S. A., 1928. Pp. 712.

This comprehensive work might well be titled a history of psychology. It is picturesque in style, excellently documented and well indexed. Frequent use is made of schematic representations, which serve both to clarify and summarize the content. While one admits the value of a discussion of general problems, one questions the advisability, in a work of this scope, of devoting nine of the total of twenty-five chapters to it. Since the material is to be taken up most adequately again in the last five chapters it seems unnecessary to spend quite so much time upon it in the first four chapters. The actual history which emphasizes Aristotle's contribution to experimental psychology is contained in about half the total number of pages. The text seems crowded, particularly because it bristles with lists of names. To the student who is reading for information these lists must be meaningless and discouraging—they seem superflous, as almost every page contains excellent footnote references.

University of Illinois E. F. MÖLLER

Instinct and Personality. By A. CAMPBELL GARNETT. New York, Dodd, Mead & Co., 1928. Pp. 218.

The author is lecturer to tutorial classes in philosophy in the University of Adelaide. To the American reader the main appeal of his book will be the light thrown on the status of psychology in Australian universities. From its very nature such a treatise must deal with many problems in social psychology. Garnett's attack on the problem is practically foretold by his philosophical slant and almost total neglect of American authorities. The instincts are largely those of McDougall, with a few additions and subtractions. Recent attacks on this conception and the whole conditioned reflex literature is simply ignored. From his consideration of instincts, the author goes on to the sentiments, once again using McDougall as his guide. Two chapters are devoted to the subject of 'the unconscious,' approached from the typical psychoanalytical point of view; and Garnett ends his book, as would any philosopher, struggling with the eternal question of body and mind, by lining up in the interactionist camp.

The book is well written and carefully organized, and presents in able fashion an interesting point of view.

Colgate University G. H. ESTABROOKS

The Psychology of Abnormal People. By JOHN J. B. MORGAN. New York, Longmans, Green & Co., 1928. Pp. ix, 627.

The reader of this book is relieved by not having to follow the traditional lines laid out by the psychiatrists. He is made to feel that the author is describing persons, as the title indicates. The discussion of personality is not limited to the one chapter under that name, but is prominent throughout the book, especially the latter half of it. The experiences of the individual in his struggle for adjustment to the world are important factors in the development of abnormalities. Psychoanalysis is dealt with rather liberally, although the author is more careful to distinguish between theory and fact than the psychoanalysts usually are. The educational implications, including a chapter on mental hygiene, make the book of more practical value than a mere recounting and description of symptoms. The glossary of important technical words at the end of each chapter will be of great aid to the beginning student of abnormal psychology.

Duke University HOWARD EASLEY

Modern Language Prognosis Test. By MAX A. LURIA and JACOB S. ORLEANS. Yonkers-on-Hudson, World Book Co., 1928, 1930.

The test consists of actual lessons in Spanish and in French with tests following each lesson. The lessons cover the recognition of English derivatives in Spanish words, the pluralizing of nouns, the writing of simple sentences in Spanish, the use of the forms of verbs, the gender of the nouns, and the agreement of adjectives in a sentence. Correlations between the test and achievement scores range from 0.70 to 0.75.

University of North Carolina A. M. JORDAN

Viljandet i dess förhållande till Jaget och Aktiviteten. By EINER TEGEN. Upsala, Almquist & Wiksells Boktryckeri, 1928. Pp. 568.

This book is a companion volume of the author's *Moderne Willenstheorien, Eine Darstellung und Kritik*, the first volume of which appeared in 1924 and the second in 1927. But whereas the *Moderne Willenstheorien* comprised a review of important modern theories, this volume is devoted to a systematic presentation of problems inherent in those various theories, an attempt at a synthesis of their various points of view, and an effort "to find a way out of the labyrinth and arrive at a sustainable conception of the will." That the author is not so presumptuous as to assume his conclusions will constitute a final or closed system is indicated by his recognition that the subject under consideration is ambiguous and difficult of apprehension, that its meanings are changing, and that the very subject itself raises questions involving the most varied philosophical and psychological problems.

The concept of volition as action or activity is treated as one of quite general acceptance. Then follows an analysis of the psychic condition we call will, five components of which are considered in detail. These are, (1) the time element of the psychic condition, (2) the time element of activity, (3) apprehension, the feeling of effort, (4) the unity of the psychic condition, and (5) the importance of consciousness (consciousness of). Consciousness is viewed as a universal attribute of the psychic state, and implies an Ego, and self-consciousness. Various theories based on feeling, desire, choice and decision are reviewed. The author's own theory of volition is developed in particular detail. More than two hundred pages are devoted to this, and out of this mass of discussion the following salient features emerge. Volition is a psychic condition, a state of attention, which implies that along with an attentive "consciousness of" having a distinct character there also enters a "moment of feeling." This "consciousness of" is an apprehension of action in which the active Ego receives "the life and color of feeling." "One can therefore say that in volition a 'consciousness of' enters, in which feeling (ego) is placed in relation to a distinct happening." Volition is not itself the "consciousness of," but rather in volition we have an attentive "consciousness of." The complexity of feeling is recognized, and is developed as a co-existence of the quality of feeling and the apprehension of a distinct happening, involving the element of time. Volition itself is an indissoluble quality, "a distinct element of the soul."

The book contains a carefully organized and detailed table of contents, but there is no index of subjects. No bibliography is provided, but numerous and specific references appear in the foot notes. As a theoretical treatise this work is preëminent. It provides an important contribution to psychological literature in a field in which American psychologists have not distinguished themselves. That this contribution may be more pronounced in making available a vast amount of material than in any finality of conclusion in no way detracts from its value. It may, however, be rather disconcerting for some of us to find that reference to the soul has not altogether disappeared from psychological literature.

Syracuse University ERNST THELIN